Scientific Culture
& Other Essays

By J. P. Cooke

SCIENTIFIC CULTURE,

AND OTHER ESSAYS.

BY

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TO

MY ASSOCIATES

IN

THE CHEMICAL LABORATORY

OF

HARVARD COLLEGE

THIS VOLUME

IS

AFFECTIONATELY DEDICATED.

PREFACE.

The essays collected in this volume, although written for special occasions without reference to each other, have all a bearing on the subject selected as the title of the volume, and are an outcome of a somewhat large experience in teaching physical science to college students. Thirty years ago, when the writer began his work at Cambridge, instruction in the experimental sciences was given in our American colleges solely by means of lectures and recitations. Chemistry and Physics were allowed a limited space in the college curriculum as branches of useful knowledge, but were regarded as wholly subordinate to the classics and mathematics as a means of education; and as physical science was then taught, there can be no question that the accepted opinion was correct. Experimental science can never be

made of value as a means of education unless taught by its own methods, with the one great aim in view to train the faculties of the mind so as to enable the educated man to read the Book of Nature for himself.

Since the period just referred to, the example early set at Cambridge of making the student's own observations in the laboratory or cabinet the basis of all teaching, either in experimental or natural history science, has been generally followed. But in most centers of education the old traditions so far survive that the great end of scientific culture is lost in attempting to conform even laboratory instruction to the old academic methods of recitations and examinations. These, as usually conducted, are simply hindrances in a course of scientific training, because they are no tests of the only ability or acquirement which science values, and therefore set before the student a false aim. To point out this error, and to claim for science teaching its appropriate methods, was one object of the writer in these essays.

It is, however, too often the case that, in following out our theories of education, we avoid Scylla only to encounter Charybdis, and so, in specializing our courses of laboratory instruction, there is great danger of falling

into the mechanical routine of a technical art, and losing sight of those grand ideas and generalizations which give breadth and dignity to scientific knowledge. That these great truths are as important an element of scientific culture as experimental skill, the author has also endeavored to illustrate, and he has added brief notices of the lives of two noble men of science which may add force to the illustrations.

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ESSAYS.

I.

SCIENTIFIC CULTURE.

An Address delivered July 7, 1875, at the Opening of the Summer Courses of Instruction in Chemistry, at Harvard University.

You have come together this morning to begin various elementary courses of instruction in chemistry and mineralogy. As I have been informed, most of you are teachers by profession, and your chief object is to become acquainted with the experimental methods of teaching physical science, and to gain the advantages in your study which the large apparatus of this university is capable of affording.

In all this I hope you will not be disappointed. You, as teachers, know perfectly well that success must depend, first of all, on your own efforts; but, since the methods of studying Nature are so different from those with which you are familiar in literary studies, I feel that the best service I can render, in this introductory

address, is to state, as clearly as I can, the great objects which should be kept in view in the courses on which you are now entering.

By your very attendance on these courses you have given the strongest evidence of your appreciation of the value of chemical studies as a part of the system of education, and let me say, in the first place, that you have not overvalued their importance. The elementary principles and more conspicuous facts of chemistry are so intimately associated with the experience of every-day life, and find such important applications in the useful arts, that no man at the present day can be regarded as educated who is ignorant of them. Not to know why the fire burns, or how the sulphur trade affects the industries of the world, will be regarded, by the generation of men among whom your pupils will have to win their places in society, as a greater mark of ignorance than a false quantity in Latin prosody or a solecism in grammar.

Moreover, I need not tell you that physical science has become a great power in the world. Indeed, after religion, it is the greatest power of our modern civilization. Consider how much it has accomplished during the last century toward increasing the comforts and enlarging the intellectual vision of mankind. The railroad, the steamship, the electric telegraph, photography, gaslights, petroleum oils, coal-tar colors, chlorine bleaching, anæsthe-

sia, are a few of its recent material gifts to the world; and not only has it made one pair of hands to do the work of twenty, but it has so improved and facilitated the old industries that what were luxuries to the fathers of our republic have become necessities to our generation.

And when, passing from these material fruits, you consider the purely intellectual triumphs of physical science, such as those which have been gained with the telescope, the microscope, and the spectroscope, you can not wonder at the esteem in which these branches of study are held in this practical age of the world.

Now, these immense results have been gained by the application to the study of Nature of a method which was so admirably described by Lord Bacon in his "Novum Organon," and which is now generally called the experimental method. What we observe in Nature is an orderly succession of phenomena. The ancients speculated about these phenomena as well as ourselves, but they contented themselves with speculations, animating Nature with the products of their wild fancies. Their great master, Aristotle, has never been excelled in the art of dialectics; but his method of logic applied to the external world was of very necessity an utter failure. It is frequently said, in defense of the exclusive study of the records of ancient learning, that they are the prod-

ucts of thinking, loving, and hating men, like ourselves, and it is claimed that the study of science can never rise to the same nobility because it deals only with lifeless matter. But this is a mere play on words, a repetition of the error of the old schoolmen.

Physical science is noble because it does deal with thought, and with the very noblest of all thought. Nature at once manifests and conceals an Infinite Presence: her methods and orderly successions are the manifestations of Omnipotent Will; her contrivances and laws the embodiment of Omniscient Thought. The disciples of Aristotle so signally failed simply because they could see in Nature only a reflection of their idle fancies. The followers of Bacon have so gloriously succeeded because they approached Nature as humble students, and, having first learned how to question her, have been content to be taught and not sought to teach. The ancient logic never relieved a moment of pain, or lifted an ounce of the burden of human misery. The modern logic has made a very large share of material comfort the common heritage of all civilized men.

In what, then, does this Baconian system consist? Simply in these elements: 1. Careful observation of the conditions under which a given phenomenon occurs; 2. The varying of these conditions by experiments, and observing the effects produced by the variation. We thus

find that some of the conditions are merely accidental circumstances, having no necessary connection with the phenomenon, while others are its invariable antecedent. Having now discovered the true relations of the phenomenon we are studying, a happy guess, suggested probably by analogy, furnishes us with a clew to the real causes on which it depends. We next test our guess by further experiments. If our hypothesis is true, this or that must follow; and, if in all points the theory holds, we have discovered the law of which we are in search. If, however, these necessary inferences are not realized, then we must abandon our hypothesis, make another guess, and test that in its turn. Let me illustrate by two well-known examples:

The, of old, universally accepted principle that all living organisms are propagated by seeds or germs (*omnia ex ovo*) has been seriously questioned by a modern school of naturalists. Various observers have maintained that there were conditions under which the lower forms of organic life were developed independently of all such accessories, but other, and equally competent, naturalists, who have attempted to investigate the subject, have obtained conflicting results.

Thus it was observed that certain low forms of life were quite constantly developed in beef juice that had been carefully prepared and hermetically sealed in glass

flasks, even after these flasks had been exposed for a long time to the temperature of boiling water. "Here," proclaims the new school, "is unmistakable evidence of spontaneous generation; for, if past experience is any guide, all germs must have been killed by the boiling water." "No," answer the more cautious naturalists, "you have not yet proved your point. You have no right to assume that all germs are killed at this temperature."

The experiments, therefore, were repeated under various conditions and at different temperatures, but with unsatisfactory results, until Pasteur, a distinguished French physicist, devised a very simple mode of testing the question. He reasoned thus: "If, as is generally believed, the presence of invisible spores in the air is an essential condition of the development of these lower growths, then their production must bear some proportion to the abundance of these spores. Near the habitations of animals and plants, where the spores are known to be in abundance, the development would be naturally at a maximum, and we should expect that the growth would diminish in proportion as the microscope indicated that the spores diminished in the atmosphere."

Accordingly, Pasteur selected a region in the Jura Mountains suitable for his purpose, and repeated the well-known experiment with beef juice, first at the inn of a

town at the foot of the mountains, and then at various elevations up to the bare rocks which covered the top of the ridge, a height of some eight thousand feet. At each point he sealed up beef juice in a large number of flasks, and watched the result. He found that while in the town the animalcules were developed in almost all the flasks, they appeared only in two or three out of a hundred cases where the flasks had been sealed at the top of the mountain, and to a proportionate extent in those sealed at the intermediate elevations. What, now, did these experiments prove? Simply this, that the development of these organic forms was in direct proportion to the number of germs in the air. It did not settle the question of spontaneous generation, but it showed that false conclusions had been deduced from the experiments which had been cited to prove it.

A still more striking illustration of the same method of questioning Nature is to be found in the investigation of Sir Humphry Davy, on the composition of water. The voltaic battery which works our telegraphs was invented by Volta in 1800; and later, during the same year, it was discovered in London, by Nicholson and Carlisle, that this remarkable instrument had the power of decomposing water. These physicists at once recognized that the chief products of the action of the battery on water were hydrogen and oxygen gases, thus confirming

the results of Cavendish, who, in 1781, had obtained water by combining these elementary substances; oxygen having been previously discovered in 1775, and hydrogen, at least, as early as 1766. It was, however, very soon also observed that there were always formed by the action of the battery on water, besides these aëriform products, an alkali and an acid, the alkali collecting around the negative pole, and the acid around the positive pole of the electrical combination. In regard to the nature of this acid and alkali, there was the greatest difference of opinion among the early experimenters on this subject. Cruickshanks supposed that the acid was nitrous acid, and the alkali ammonia. Desormes, a French chemist, attempted to prove that the acid was muriatic acid; while Brugnatelli asserted that a new and peculiar acid was formed, which he called the electric acid.

It was in this state of the question that Sir Humphry Davy began his investigation. From the analogies of chemical science, as well as from the previous experiments of Cavendish and Lavoisier, he was persuaded that water consisted solely of oxygen and hydrogen gases, and that the acid and alkali were merely adventitious products. This opinion was undoubtedly well founded; but, great disciple of Bacon as he was, Davy felt that his opinion was worth nothing unless substantiated by ex-

perimental evidence, and accordingly he set himself to work to obtain the required proof.

In Davy's first experiments the two glass tubes which he used to contain the water were connected together by an animal membrane, and he found, on immersing the poles of his battery in their respective tubes, that, besides the now well-known gases, there were really formed muriatic acid in one tube, and a fixed alkali in the other. Davy at once, however, suspected that the acid and alkali came from common salt contained in the animal membrane, and he therefore rejected this material and connected the glass tubes by carefully washed cotton fiber, when, on submitting the water as before to the action of the voltaic current, and continuing the experiment through a great length of time, no *muriatic* acid appeared; but he still found that the water in the one tube was strongly alkaline, and in the other strongly acid, although the acid was chiefly, at least, nitrous acid. A part of the acid evidently came from the animal membrane, but not the whole, and the source of the alkali was as obscure as before.

Davy then made another guess. He knew that alkali was used in the manufacture of glass; and it occurred to him that the glass of the tubes, decomposed by the electric current, might be the origin of the alkali in his experiments. He therefore substituted for the glass

tubes cups of agate, which contains no alkali, and repeated the experiment, but still the troublesome acid and alkali appeared. Nevertheless, he said, it is possible that these products may be derived from some impurities existing in the agate cups, or adhering to them; and so, in order to make his experiments as refined as possible, he rejected the agate vessels and procured two conical cups of pure gold, but, on repeating the experiments, the acid and alkali again appeared.

And now let me ask who is there of us who would not have concluded at this stage of the inquiry that the acid and alkali were essential products of the decomposition of water? But not so with Davy. He knew perfectly well that all the circumstances of his experiments had not been tested, and until this had been done he had no right to draw such a conclusion. He next turned to the water he was using. It was distilled water, which he supposed to be pure, but still, he said, it is possible that the impurities of the spring-water may be carried over to a slight extent by the steam in the process of distillation, and may therefore exist in my distilled water to a sufficient amount to have caused the difficulty. Accordingly, he evaporated a quart of this water in a silver dish, and obtained seven-tenths of a grain of dry residue. He then added this residue to the small amount of water in the gold cones and again repeated the experiment.

The proportion of alkali and acid was sensibly increased.

You think he has found at last the source of the acid and alkali in the impurities of the water. So thought Davy, but he was too faithful a disciple of Bacon to leave this legitimate inference unverified. Accordingly, he repeatedly distilled the water from a silver alembic until it left absolutely no residue on evaporation, and then with water which he knew to be pure, and contained in vessels of gold from which he knew it could acquire no taint, he still again repeated the already well-tried experiment. He dipped his test-paper into the vessel connected with the positive pole, and the water was still decidedly acid. He dipped the paper into the vessel connected with the negative pole, and the water was still alkaline.

You might well think that Davy would have been discouraged here. But not in the least. The path to the great truths which Nature hides often leads through a far denser and a more bewildering forest than this; but then there is not infrequently a "blaze" on the trees which points out the way, although it may require a sharp eye in a clear head to see the marks. And Davy was well enough trained to observe a circumstance which showed that he was now on the right path and heading straight for the goal.

On examining the alkali formed in this last experi-

ment, he found that it was not, as before, a fixed alkali, soda or potash, but the volatile alkali ammonia. Evidently the fixed alkali came from the impurities of the water, and when, on repeating the experiment with pure water in agate cups or glass tubes, the same results followed, he felt assured that so much at least had been established. There was still, however, the production of the volatile alkali and of nitrous acid to be accounted for. As these contain only the elements of air and water, Davy thought that possibly they might be formed by the combination of hydrogen at the one pole and of oxygen at the other with the nitrogen of the air, which was necessarily dissolved in the water. In order, therefore, to eliminate the effect of the air, he again repeated the experiment under the receiver of an air-pump from which the atmosphere had been exhausted, but still the acid and alkali appeared in the two cups.

Davy, however, was not discouraged by this, for the "blazes" on the trees were becoming more numerous, and he now felt sure that he was fast approaching the end. He observed that the quantity of acid and alkali had been greatly diminished by exhausting the air, and this was all that could be expected, for, as Davy knew perfectly well, the best air-pumps do not remove all the air. He therefore, for the last experiment, not only exhausted the air, but replaced it with pure hydrogen, and then ex-

hausted the hydrogen and refilled the receiver with the same gas several times in succession, until he was perfectly sure that the last traces of air had been as it were washed out. In this atmosphere of pure hydrogen he allowed the battery to act on the water, and not until the end of twenty-four hours did he disconnect the apparatus. He then dips his test-paper into the water connected with the positive pole, and there is no trace of acid; he dips it into the water at the negative pole, and there is no alkali; and you may judge with what satisfaction he withdraws those slips of test-paper, whose unaltered surfaces showed that he had been guided at last to the truth, and that his perseverance had been rewarded.

The fame of Sir Humphry Davy rests on his discovery of the metals of the alkalies and earths which first revealed the wonderful truth that the crust of our globe consists of metallic cinders; but none of these brilliant results show so great scientific merit or such eminent power of investigating Nature as the experiments which I have just detailed. I have not, however, described them here for the purpose of glorifying that renowned man. His honored memory needs no such office at my hands. My only object was to show you what is meant by the Baconian method of science, and to give some idea of the nature of that modern logic which within the

last fifty years has produced more wonderful transformations in human society than the author of Aladdin ever imagined in his wildest dreams. In this short address I can of course give you but a very dim and imperfect idea of what I have called the Baconian system of experimental reasoning. Indeed, you can not form any clear conception of it, until in some humble way you have attempted to use the method, each one for himself, and you have come here in order that you may acquire such experience.

My object, however, will be gained if these illustrations serve to give emphasis to the following statements, which I feel I ought to make at the opening of these courses of instruction—statements which have an especial appropriateness in this place, since I am addressing teachers, who are in a position to exert an important influence on the system of education in this country.

In the first place, then, I must declare my conviction that no educated man can expect to realize his best possibilities of usefulness without a practical knowledge of the methods of experimental science. If he is to be a physician, his whole success will depend on the skill with which he can use these great tools of modern civilization. If he is to be a lawyer, his advancement will in no small measure be determined by the acuteness with which he can criticise the manner in which the same tools have

been used by his own or his opponent's clients. If he is to be a clergyman, he must take sides in the great conflict between theology and science which is now raging in the world, and, unless he wishes to play the part of the doughty knight Don Quixote, and think he is winning great victories by knocking down the imaginary adversaries which his ignorance has set up, he must try the steel of his adversary's blade.

Let me be fully understood. It is not to be expected or desired that many of our students should become professional men of science. The places of employment for scientific men are but few, and more in the future than in the past they will naturally be secured by those whom Nature has endowed with special aptitudes or tastes—usually the signs of aptitudes—to investigate her laws. That our country will always offer an honorable career to her men of genius, we have every reason to expect, and these born students of Nature will usually follow the plain indications of Providence without encouragement or direction from us.

It is different, however, with the great body of earnest students who are conscious of no special aptitudes, but who are desirous of doing the best thing to fit themselves for usefulness in the world; and I feel that any system of education is radically defective which does not comprise a sufficient training in the methods of ex-

perimental science to make the mass of our educated men familiar with this tool of modern civilization: so that, when, hereafter, new conquests over matter are announced and great discoveries are proclaimed, they may be able not only to understand but also to criticise the methods by which the assumed results have been reached, and thus be in a position to distinguish between the true and the false. Whether we will or not, we must live under the direction of this great power of modern society, and the only question is whether we will be its ignorant slave or its intelligent servant.

In the second place, it seems fitting that I should state to you what I regard as the true aims to be kept in view in a course of scientific study, and to give my reasons for the methods we have adopted in arranging the courses you are about beginning.

In our day there has arisen a warm discussion as to the relative claims of two kinds of culture, and attempts are made to create an antagonism between them. But all culture is the same in spirit. Its object is to awaken and strengthen the powers of the mind; for these, like the muscles of the body, are developed and rendered strong and active only by exercise; while, on the other hand, they may become atrophied from mere want of use. Science culture differs in its methods from the old classical culture, but it has the same spirit and the same ob-

ject. You must not, therefore, expect me to advocate the former at the expense of the latter; for, although I have labored assiduously during a quarter of a century to establish the methods of science teaching which have now become general, I am far from believing that they are the only true modes of obtaining a liberal education. So far from this, if it were necessary to choose one of two systems, I should favor the classical; and why?

Language is the medium of thought, and can not be separated from it. He who would think well must have a good command of language, and he who has the best command of language I am almost tempted to say will think the best. For this reason a certain amount of critical study of language is essential for every educated man, and such study is not likely to be gained except through the great ancient languages; the advocates of classical scholarship frequently say, can not be gained. I am not ready to accept this dictum; but I most willingly concede that in the present state of our schools it is not likely to be gained. I never had any taste myself for classical studies; but I know that I owe to the study a great part of the mental culture which has enabled me to do the work that has fallen to my share in life.

But, while I concede all this, I do not believe, on the other hand, that the classical is the only effective method of culture; you evidently do not think so, for you would

not be here if you did. But, in abandoning the old tried method, which is known to be good, for the new, you must be careful that you gain the advantages which the new offers; and you will not gain the new culture you seek unless you study science in the right way. In the classical departments the methods are so well established, and have been so long tested by experience, that there can hardly be a wrong way. But in science there is not only a wrong way, but this wrong way is so easy and alluring that you will most certainly stray into it unless you strive earnestly to keep out of it. Hence I am most anxious to point out to you the right way, and do what I can to keep you in it; and you will find that our courses and methods have been devised with this object.

When advocating in our mother University of Cambridge, in Old England, the claims of scientific culture, I was pushed with an argument which had very great weight with the eminent English scholars present, and which you will be surprised to learn was regarded as fatal to the success of the science "triposes" then under debate. The argument was that the experimental sciences could not be made the subjects of competitive examinations. Some may smile at such an objection; but, as viewed from the English standpoint, there was really a great deal in it, and the argument brought out the radical difference between scientific and classical culture.

The old method of culture may be said to have culminated in the competitive examinations of the English universities. We have no such examinations here. Success depends not simply on knowing your subject thoroughly, but on having it at your fingers' ends, and those fingers so agile that they can accomplish not only a prodigious amount of work in a short time, but can do this work with absolute accuracy. For the only approach we make to an experience of this kind, we must look to our athletic contests. It may of course be doubted whether the ability, once in a man's life, to perform such mental feats, is worth what it costs. Still it implies a very high degree of mental culture, and it is perfectly certain that the experimental sciences give no field for that sort of mental prize-fights. It is easy to prepare written examinations which will show whether the students have been faithful to their work, but they can not be adapted to such competitions as I have described without abandoning the true object of science culture. The ability of the scientific student can only be shown by long-continued work at the laboratory table, and by his success in investigating the problems which Nature presents.

We have here struck the true key-note of the scientific method. The great object of all our study should be to study Nature, and all our methods should be directed to this one object. This aim alone will ennoble our schol-

arship as students, and will give dignity to our scientific calling as men of science. It is this high aim, moreover, which vindicates the worth of the mode of culture we have chosen. What is it that ennobles literary culture but the great minds which, through this culture, have honored the nations to which they belong?

The culture we have chosen is capable of even greater things; not because science is nobler than art, for both are equally noble—it is the thought, the conception, which ennobles, and I care not whether it be attained through one kind of exercise of the mental faculties or another—but we are capable of grander and nobler thoughts than Plato, Cicero, Shakespeare, or Newton, because we live in a later period of the world's history, when, through science, the world has become richer in great ideas. It is, I repeat, the great thought which ennobles, and it ennobles because it raises to a higher plane that which is immortal in our manhood.

If I have made my meaning clear, and if you sympathize with my feelings, you will understand why I regard culture as so important to the individual and to the nation. The works of Shakespeare and of Bacon are of more value to England to-day than the memories of Blenheim or Trafalgar; and those great minds will still be living powers in the world when Marlborough and Nelson are only remembered as historical names.

I therefore believe that it is the first duty of a country to foster the highest culture, and that it should be the aim of every scholar to promote this culture both by his own efforts and his active influence. A nation can become really great in no other way. We live in a country of great possibilities; and the danger is that, as with many men I have known in college, of great potential abilities, the greatness will end where it begins. The scholars of the country should have but one voice in this matter, and urge upon the government and upon individuals the duty of encouraging and supporting mental culture for its own sake.

The time has passed when we can afford to limit the work of our higher institutions of learning to teaching knowledge already acquired. Henceforth the investigation of unsolved problems, and the discovery of new truth, should be one of the main objects at our American universities, and no cost grudged which is required to maintain at them the most active minds, in every branch of knowledge which the country can be stimulated to produce.

I could urge this on the self-interest of the nation as an obvious dictate of political economy. I could say, and say truly, that the culture of science will help us to develop those latent resources of which we are so proud; will enable us to grow two blades of grass

where one grew before; to extract a larger percentage of metal from our ores; to economize our coal, and in general to direct our waiting energies so that they may produce a more abundant pecuniary reward. I could tell of Galvani studying for twenty long years, to no apparent purpose, the twitching of frogs' hind-legs, and thus sowing the seed from which has sprung the greatest invention of modern times. Or, if our Yankee impatience would be unwilling to wait half a century for the fruit to ripen, I could point to the purely theoretical investigations of organic chemistry, which in less than five years have revolutionized one of the great industries of Europe, and liberated thousands of acres for a more beneficent agriculture. This is all true, and may be urged properly if higher considerations will not prevail. It is an argument I have used in other places, but I will not use it here; although I gladly acknowledge the Providence which brings at last even material fruits to reward conscientious labor for the advancement of knowledge and the intellectual elevation of mankind. I would rather point to that far greater multitude who worked in faith for the love of knowledge, and who ennobled themselves and ennobled their nation, not because they added to its material prosperity, but because they made themselves and made their fellows more noble men.

I come back now again to the moral of all this, to

urge upon you, as the noblest patriotism and the most enlightened self-interest, the duty of striving for yourselves and encouraging in others the highest culture in the studies you have chosen, and this culture with one end in view, to advance knowledge. I am far, of course, from advising you to grapple immaturely with unsolved problems, or, when you have gained the knowledge with which you can dare to venture from the beaten track, to undertake work beyond your power. Many a young scientific man has suffered the fate of Icarus in attempting to soar too high. Moreover, I am far from expecting that all or many of you will ever have the opportunity of going beyond the well-explored fields of knowledge; but you can all have the aim, and that aim will make your work more worthy and more profitable to yourselves. Every American boy can not be President of the United States, but if, as our English cousins allege, he believes that he can be, the very belief makes him an abler man.

We have dwelt long enough on these generalities, and it is time to come down to commonplaces, and to inquire what are the essential conditions of this scientific culture which shall fit us to investigate Nature; and the first thought that occurs to me in this connection may be expressed thus: Science presents to us two aspects, which I may call its objective and its subjective aspect. Objec-

tively it is a body of facts, which we have to observe, and subjectively it is a body of truths, conclusions, or inferences, deduced from these facts; and the two sides of the subject should always be kept in view.

I propose next to say a few words in regard to each of these two aspects of our study, and in regard to the best means of training our faculties so as to work successfully in each sphere. First, then, success in the observation of phenomena implies three qualities at least, namely, quickness and sharpness of perception, accuracy in details, and truthfulness; and on its power to cultivate these qualities a large part of the value of science, as a means of education, depends.

To begin with the cultivation of our perceptions. We are all gifted with senses, but how few of us use them to the best advantage! "We have eyes and see not"; for, although the light paints the picture on the retina, our dull perceptions give no attention to the details, and we retain only a confused impression of what has passed before our eyes. "But how," you may ask, "are we to cultivate this sharpness of perception?" I answer, only by making a conscious effort to fix our attention on the objects we study until the habit becomes a second nature. I have often noticed, with surprise, the power which uneducated miners frequently possess of recognizing many minerals at sight. This

they have acquired by long experience and close familiarity with such objects, and such power of observation is with them so purely a habit that they are frequently unable to state clearly the grounds on which their conclusions are based. They recognize the minerals by what in common language is called their "looks" and they notice delicate differences in the "looks" to which most men are blind. It is, however, the business of the scientific mineralogist to analyze these "looks," and to point out in what the differences consist; so that by fixing his attention on these points the student may gain, by a few hours' study, the power which the miner acquires only after long experience.

The chief difficulty, however, which we find in teaching mineralogy is, that the students do not readily see the differences when they are pointed out, or, if they see them, do not remember them with sufficient precision to render their subsequent observations conclusive and precise. This either arises from a failure to cultivate the powers of observation in childhood, or the subsequent blunting of them by disuse. The ladies will scout the idea that a brooch of cut-glass is as ornamental as one of diamond, and yet I venture to assert that there is not one person in fifty, at least of those who have not made a study of the subject, who can tell the difference between the two. The external ap-

pearance depends simply on what we call lustre. The lustre of glass is vitreous, that of the diamond adamantine; and I know of no other distinction which it is more difficult for students to recognize than this. Those of you who study mineralogy will experience this difficulty, and it can be overcome only by giving careful attention to the subject. The teacher can do nothing more than put in your hands the specimens which illustrate the point, and you must study these specimens until you see the difference. It is a question of sight, not of understanding, and all the optical theories of the cause of the lustre will not help you in the least toward seeing the difference between diamond and glass, or anglesite and heavy spar.

Another illustration of the same fact is the constant failure of students to distinguish by the eye alone between the two minerals called copper-glance and gray copper. There is a difference of color and lustre which, although usually well marked, it requires an educated eye to distinguish.

Mineralogy undoubtedly demands a more careful cultivation of the perceptions than the other branches of chemistry; but still you will find abundant practice for close observation in them all. I have often known students to reach erroneous results in qualitative analysis by mistaking a white precipitate in a colored liquid for a

colored precipitate, or by not attending to similar broad distinctions, which would have been obvious to any careful observer; and so in quantitative analysis, mere delicacy of touch or handling is a great element of success.

But I must pass on to speak of the importance in the study of Nature of accuracy in detail, which is the second condition of successful observation of which I spoke. We must cultivate not only accuracy in observing details, but also accuracy in following details which have been laid down by others for our guidance. In science we can not draw correct conclusions from our premises unless we are sure that we have all the facts, and what seemed at first an unimportant detail often proves to be the determining condition of the result; and, again, if we are told that under certain conditions a certain sign is the proof of the presence of a certain substance, we have no right to assume that the sign is of any value unless the conditions are fulfilled. A black precipitate, for example, obtained under certain conditions, is a proof of the presence of nickel, but we can not assert that we have found nickel unless we have followed out those details in every particular.

Of course, we must avoid empiricism as far as we can. We must seek to learn the reasons of the details, and such knowledge will not only render our work intelligent, but will also frequently enable us to judge

how far the details are essential, and to what extent our processes may be varied with safety. We must also avoid trifling, and, above all, "the straining at a gnat and swallowing a camel," as is the habit with triflers. Large knowledge and good judgment will avoid all such errors; but, if we must choose between fussiness and carelessness, the first is the least evil. Slovenly work means slovenly results, and habits of carefulness, neatness, and order produce as excellent fruits in the laboratory as in the home.

Last in order but first in importance of the conditions of successful observation, mentioned above, stands truthfulness. Here you may think I am approaching a delicate subject, of which even to speak might seem to cast a reproach. But not so at all. I am not speaking here of conscious deception, for I assume that no one who aspires to be a student of Nature can be guilty of that. But I am speaking of a quality whose absence is not necessarily a mark of sinfulness, but whose possession, in a high degree, is a characteristic of the greatest scientific talent. As every lawyer knows, he is a rare man whose testimony is not colored by his interests, and a very large amount of self-deception is compatible with conscious honesty of purpose.

So among scientific students the power to keep the mind unbiased, and not to color our observations in the

least degree, is one of the rarest as it is one of the noblest of qualities. It is a quality we must strive after with all our might, and we shall not attain it unless we strive. Remember, our observations are our data, and, unless accurate, everything deduced from them must have the taint of our deception. We can not deceive Nature, however much we may deceive ourselves; and there is many a student who would cut off his right hand rather than be guilty of a conscious untruth, who is yet constantly untruthful to himself. Every year students of mineralogy present to me written descriptions of mineral specimens which particularize, as observed, characters that do not appear on the specimen given them to determine, although they may be the correct characters of some other mineral.

There is usually no want of honesty in this, but, deceived by some accident, the student has made a wrong guess, and then imagined that he saw on the specimen those characters which he knew from the descriptions ought to appear on the assumed mineral. So, also, it not unfrequently happens that a student in qualitative analysis, who has obtained some hints in regard to the composition of his solution, will torture his observations until they seem to him to confirm his erroneous inferences; and again the student in quantitative analysis, who finds out the exact weight he ought to obtain, is often insensibly

influenced by this knowledge—in the washing and ignition of his precipitate, or in some other way—and thus obtains results whose only apparent fault may be a too close agreement with theory, but which, nevertheless, are not accurate because not true. It is evident how fatal such faults as these must be to the investigation of truth, and they are equally destructive of all scientific scholarship. Their effect on the student is so marked that, although he may deceive himself, he will rarely deceive his teacher. That he should lose confidence in his own results is, to the teacher, one of the most marked indications of such false methods of study, but the student usually refers his want of success to any cause but the real one—his own untruthfulness. He will complain of the teacher, or of the methods of instruction, and may even persuade himself that all scientific results are as uncertain as his own. As I have said, mere ordinary truthfulness, which spurns any conscious deception, will not save us from falling into such faults. Our scientific study demands a much higher order of truthfulness than this. We should so love the truth above all price as to strive for it with single-hearted and unswerving purpose. We must be constantly on our guard to avoid any circumstance which would tend to bias our minds or warp our judgments, and we must make the attainment of the truth our sole motive, guide, and end.

It remains for me, before closing this address, to say a few words on what I have called the subjective aspect of scientific study. Science offers us not only a mass of phenomena to be observed, but also a body of truths which have been deduced from these observations; and, without the power of drawing correct inferences from the data acquired, exact observations would be of little value. I have already described the inductive method of reasoning, and illustrated it by two noteworthy examples, and, in a humbler measure, we must apply the same method in our daily work in the laboratory. We must learn how to vary our experiments so as to eliminate the accidental circumstances, and make evident the essential conditions of the phenomena we are studying. Such power can only be acquired by practice, and a somewhat long experience in active teaching has convinced me that there is no better means of training this logical faculty than the study of qualitative chemical analysis in which many of you are to engage.

The results of the processes of qualitative analysis are perfectly definite and trustworthy; but they are only reached by following out the indications of experiments which are frequently obscure, and even apparently contradictory; reconciling by new experiments the seeming discrepancies, and, at last, having eliminated all other possible causes of the phenomena observed, dis-

covering the true nature of the substances under examination.

The study of mineralogy affords an almost equally good practice, although in a somewhat different form. By comparing carefully many specimens of the same mineral, you learn to distinguish the accidental from the essential characters, and on this distinction you must base your inferences in regard to the nature of the specimens you may be called upon to determine. A single remark occurs to me which may aid you in cultivating this scientific logic.

Do not attempt to reason on insufficient data. Multiply your observations or experiments, and when your premises are ample, the conclusion will generally take care of itself. Are you in doubt in regard to a mineral specimen? Repeat your observations again and again, multiply them with the aid of the blow-pipe or goniometer, compare the specimen with known specimens which it resembles, until either your doubts are removed or you are satisfied that you are unequal to the task; and remember that, in many cases, the last is the only honest conclusion.

Are you in doubt in regard to the reactions of the substance you are analyzing, whether they are really those of a metal you suspect to be present? Do not rest in such a frame of mind, and, above all, do not try to re-

move the doubt by comparing your experience with that of your neighbor, but multiply your own experiments; procure some compound of the metal, and compare its reactions with those you have observed until you reach either a positive or a negative result.

Remember that the way to remove your doubts is to widen your own knowledge, and not to depend on the knowledge of others. When your knowledge of the facts is ample, your inferences will be satisfactory, and then an unexplained phenomenon is the guide to a new discovery. Do not be discouraged if you have to labor long in the dark before the day begins to dawn. It will at last dawn to you, as it has dawned to others before, and, when the morning breaks, you will be satisfied with the result of your labor.

Moreover, I feel confident that such experience will very greatly tend to increase your appreciation of the value of scientific studies in training the reasoning faculties of the mind. This, as every one must admit, is the best test of their utility in a scheme of education, and it is precisely here that I claim for them the very highest place. It has generally been admitted that mathematical studies are peculiarly well adapted to train the logical faculties, but still many persons have maintained that, since the mathematics deal wholly with absolute certainties, an exclusive devotion to this class of subjects unfits

the mind for weighing the probable evidence by which men are chiefly guided in the affairs of life.

But, without attempting to discuss this question, on which much might be said on both sides, it is certain that no such objection can be urged against the study of the physical sciences if conducted in the manner I have attempted to describe. These subjects present to the consideration of the student every degree of probable evidence, accustoming him to weigh all the evidence for or against a given conclusion, and to reject or to provisionally accept only on the balance of probabilities. Moreover, in practical science, the student is taught to follow out a chain of probable evidence with care and caution, to eliminate all accidental phenomena, and supply, by experiment or observation, the missing links, until he reaches the final conclusion—an intellectual process which, though based wholly on probable evidence, may have all the force and certainty of a mathematical demonstration.

Indeed, that highly valued scientific acumen and skill which enables the student to brush away the accidental circumstances by which the laws of Nature are always concealed until the truth stands out in bold relief, is but a higher phase of the same talent which marks professional skill in all the higher walks of life. The physician who looks through the external symptoms of

his patient to the real disease which lurks beneath; the lawyer, who disentangles a mass of conflicting testimony, and follows out the truth successfully to the end; the statesman, who sees beneath the froth of political life the great fundamental principles which will inevitably rule the conduct of the state, and thus foresees and provides for the coming change; the general, who discovers amid the confusion of the battlefield the weak point of his enemy's front; the merchant, even, who can interpret the signs of the unsettled market—employ the same faculty, and frequently in not a much lower degree, that discovered the law of gravitation, and which, since the days of Newton, has worked so successfully to unveil the mysteries of the material creation.

Moreover, I hope, my friends, that you will come to value scientific studies, not simply because they cultivate the perceptive and reasoning faculties, but also because they fill the mind with lofty ideals, elevated conceptions, and noble thoughts. Indeed, I claim that there is no better school in which to train the æsthetical faculties of the mind, the tastes, and the imagination, than the study of natural science.

The beauty of Nature is infinite, and the more we study her works the more her loveliness unfolds. The upheaved mountain, with its mantle of eternal snow; the majestic cataract, with its whirl and roar of waters;

the sunset cloud, with its blending of gorgeous hues, lose nothing of their beauty for him who knows the mystery they conceal. On the contrary, they become, one and all, irradiated by the Infinite Presence which shines through them, and fill the mind with grander conceptions and nobler ideas than your uneducated child of Nature could ever attain.

Remember that I am not recommending an exclusive devotion to the natural sciences. I am only claiming for them their proper place in the scheme of education, and I do not, of course, deny the unquestionable value of both the ancient and the modern classics in cultivating a pure and elevated taste. But I do say that the poet-laureate of England has drawn a deeper inspiration from Nature interpreted by science than any of his predecessors of the classical school; and I do also affirm that the pre-Raphaelite school of painting, with all its grotesque mimicry of Nature, embodies a truer and purer ideal than that of any Roman fable or Grecian dream.

And what shall we say of the imagination? Where can you find a wider field for its exercise than that opened by the discoveries of modern science? And as the mind wanders over the vast expanse, crossing boundless spaces, dwelling in illimitable time, witnessing the displays of immeasurable power, and studying the adapta-

tions of Omniscient skill, it lives in a realm of beauty, of wonder, and of awe, such as no artist has ever attained to in word, in sound, in color, or in form. And if such a life does not lead man to feel his own dependence, to yearn toward the Infinite Father, and to rest on the bosom of Infinite Love, it is simply because it is not the noble in intellect, not the great in talent, not the profound in knowledge, not the rich in experience, not the lofty in aspiration, not the gifted in imagery, but solely the pure in heart, who see God.

Such, then, is a very imperfect presentation of what I believe to be the value of scientific studies as a means of education. In what I have stated I have implied that, for these studies to be of any real value, the end must be constantly kept in view, and everything made subservient to the one great object.

To study the natural sciences merely as a collection of interesting facts which it is well for every educated man to know, seldom serves a useful purpose. The young mind becomes wearied with the details, and soon forgets what it has never more than half acquired. The lessons become an exercise of the memory and of nothing more; and if, as is too frequently the case, an attempt is made to cram the half-formed mind in a single school-year with an epitome of half the natural sciences—natural philosophy, astronomy, and chemistry, physiology,

zoölogy, botany, and mineralogy, following each other in rapid succession—these studies become a great evil, an actual nuisance, which I should be the first to vote to abate. The tone of mind is not only not improved, but seriously impaired, and the best product is a superficial, smattering smartness, which is the crying evil not only of our schools but also of our country.

In order that the sciences should be of value in our educational system, they must be taught more from things than from books, and never from books without the things. They must be taught, also, by real living teachers, who are themselves interested in what they teach, are interested also in their pupils, and understand how to direct them aright. Above all, the teach ers must see to it that their pupils study with the understanding, and not solely with the memory, not permitting a single lesson to be recited which is not thoroughly understood, taking the greatest care not to load the memory with any useless lumber, and eschewing merely memorized rules as they would deadly poison. The great difficulty against which the teachers of natural science have to contend in the colleges are the wretched treadmill habits the students bring with them from the schools. Allow our students to memorize their lessons, and they will appear respectably well, but you might as easily remove a mountain as to make many of them think. They

will solve an involved equation of algebra readily enough so long as they can do it by turning their mental crank, when they will break down on the simplest practical problem of arithmetic which requires of them only thought enough to decide whether they shall multiply or divide.

Many a boy of good capabilities has been irretrievably ruined, as a scholar, by being compelled to learn the Latin grammar by rote at an age when he was incapable of understanding it; and I fear that schools may still be found where young minds are tortured by this stupefying exercise. Those of us who have faith in the educational value of scientific studies are most anxious that the students who resort to our colleges should be as well fitted in the physical sciences as in the classics, for otherwise the best results of scientific culture can not be expected. As it is, our students come to the university, not only with no preparation in physical science, but with their perceptive and reasoning faculties so undeveloped that the acquisition of the elementary principles of science is burdensome and distasteful; and good scholars, who are ambitious of distinction, can more readily win their laurels on the old familiar track than on an untried course of which they know nothing, and for which they must begin their training anew.

We have improved our system of instruction in the

college as fast as we could obtain the means, but we are persuaded that the best results can not be reached without the coöperation of the schools. We feel, therefore, that it is incumbent upon us, in the first place, to do everything in our power to prove to the teachers of this country how great is the educational value of the physical sciences, when properly taught; and secondly, to aid them in acquiring the best methods of teaching these subjects. It is with such aims that our summer courses have been instituted, and your presence here in such numbers is the best evidence that they have met a real want of the community. We welcome you to the university and to such advantages as it can afford, and we shall do all in our power to render your brief residence here fruitful, both in experience and in knowledge; hoping, also, that the university may become to you, as she has to so many others, a bright light shining calmly over the troubled sea of active life, ever suggesting lofty thoughts, encouraging noble endeavors, and inciting all her children to work together toward those great ends, the advancement of knowledge and the education of mankind.

II.

THE NOBILITY OF KNOWLEDGE.

An Address delivered before the Free Institute at Worcester, Massachusetts, July 28, 1874.

WITHIN a comparatively few years schools for the instruction of artisans have become a prominent feature in the educational systems both of this country and of Europe, and seem destined to supersede the old system of apprenticeships. The establishment of these schools has been an important step in human progress, not because any great advantage has been gained in the cultivation of mechanical skill, but because here the future mechanic acquires culture of the mind as well as skill of the hand. Indeed, it may be doubted whether our utilitarian age can ever successfully compete with those "elder days of art" when

> "Builders wrought with greatest care
> Each minute and unseen part."

But, if our industrial schools do not make better mechanics than the workshops of the olden time, they certainly educate better men, and, by adding to skill, knowledge, they are elevating the mechanic and ennobling his calling.

If, therefore, these schools are the representatives in our age of the workshops with their bands of apprentices in the days of yore, then that by which the schools are distinguished, that which they have added to the old system, is not art but mental culture; and therefore, when asked to address you on this occasion, I could think of no more appropriate subject than the Nobility of Knowledge.

Identified with an institution in which mental culture is the chief aim, I felt that I was asked to address a body of cultivated working-men with whom, though employed in the mechanic arts, the acquisition of knowledge was also a privilege and a pride. I felt, moreover, that a proper appreciation of the true dignity of knowledge, in itself considered, and apart from all economical considerations, is one of the great wants of our age and of our country.

"Knowledge is power." "Knowledge is wealth." These trite maxims are sufficiently esteemed in our community, and need not that they be enforced by any one. So far as knowledge will yield immediate distinction or gain, it is sought and fostered by multitudes. But, when

the aim is low, the attainment is low, and too many of our students are satisfied with superficiality, if it only glitters, and with charlatanry, if it only brings gold.

Let me not be understood to depreciate the material advantages of learning. I rejoice that in this world knowledge frequently yields wealth and fame, and I should have little hope for human progress were the prizes of scholarship less than they are. Power and wealth are noble aims, and when rightly used may be the means of conferring unmeasured blessings on mankind; but I desire at this time to impress upon you, my friends, the fact that knowledge has nobler fruits than these, and that the worth of your knowledge is to be measured not by the credits it will add to your account in the ledger, or the position it may give you among men, but by the extent to which it educates your higher nature, and elevates you in the scale of manhood.

I address young men who are just entering on life, who are at an age when the mystery of our being usually presses most closely upon the soul, and whose aspirations for higher culture and clearer vision have not been deadened by the sordid damps of the world. Trust no croakers who tell you that your youthful visions are illusions, which a little contact with the real business of the world will dispel.

It is only too true that these visions will become

fainter and fainter, if you allow the cares of the world to engross your thoughts; but, unless your higher nature becomes wholly deadened, you will look back to the time when the visions were brightest, as the golden period of your life, and let me assure you that, if you only are true to the aspirations of your youth, the visions will become clearer and clearer to the last, and, as we firmly believe, will prove to be the dawn of the perfect day.

My friends, if you have seen these visions, "the nobility of knowledge" has been a reality of your experience. You know that there is a life lived in communion with the thoughts of great men or with the thoughts of God as we can read them in Nature and Revelation, which is purer and nobler than a life of money-making or political intrigue, and I would that I could so bring you to appreciate not only the nobility, but also the happiness, of such a life as to induce you to try to live it.

Do you tell me that it is only granted to a few men to become scholars, and that you have been educated for some industrial pursuit? Remember, as I said before, that it is your special privilege to have been educated, to have added knowledge to your handicraft, and that this very knowledge, if kept alive so far as you are able, will ennoble your life. Knowledge, like the fairy's wand, ennobles whatever it touches. The humblest occu-

pations are adorned by it, and without it the most exalted positions appear to true men mean and low.

Nor is it the extent of the knowledge alone which ennobles, but much more the spirit and aim with which it is cultivated, and that spirit and aim you may carry into any occupation, however engrossing, and into any condition of life, however obscure.

And let me add that what I have said is true not only of the individual, but also, and to an even greater degree, of the nation. Our people, for the most part, look upon universities and other higher institutions of learning as merely schools for recruiting the learned professions, and estimate their efficiency solely by the amount of teaching work which they perform. But, however important the teaching function of the university may be, I need not tell you that this is not its only or chief value to a community. The university should be the center of scientific investigation and literary culture, the nursery of lofty aspirations and noble thoughts, and thus should become the soul of the higher life of the nation. For this and this chiefly it should be sustained and honored, and no cost and no sacrifice can be too great which are required to maintain its efficiency; and its success should be measured by the amount of knowledge it produces rather than by the amount of instruction it imparts.

Harvard College, by cherishing and honoring the

great naturalist she has recently lost, has done more for Massachusetts than by educating hosts of commonplace professional men. The simple title of teacher, which in his last will Louis Agassiz wrote after his name, was a nobler distinction than any earthly authority could confer; but remember he was a teacher not of boys, but of men, and his influence depended not on the instruction in natural history which he gave in his lecture-room, but on his great discoveries, his far-reaching generalization, and his noble thoughts. Although that man died poor, as the world counts poverty, yet the bequest which he left to this people can not be estimated in coin.

It is a sorry confession to make, but it is nevertheless the truth, that, if we compare our American universities, in point of literary or scientific productiveness, with those of the Old World, they will appear lamentably deficient. Let me add, however, that this deficiency arises not from any want of proper aims in our scholars, but simply from the circumstance that our people do not sufficiently appreciate the value of the higher forms of literary and scientific work to bear the burden which the production necessary entails. Scholars must live, as well as other men, and in a style which is in harmony with their surroundings and cultivated tastes, and their best efforts can not be devoted to the extension of knowledge unless they are relieved from anxiety in regard to their daily bread.

In our colleges the professors are paid for teaching and for teaching only, while in a foreign university the teaching is wholly secondary, and the professor is expected to announce in his lectures the results of his own study, and not the thoughts of other men. Until the whole status of the professors in our chief universities can be changed, very little original thought or investigation can be expected, and these institutions can not become what they should be, the soul of the higher life of the nation.

It is in your power, however, to bring about this change, but the reform can be effected in only one way. You must give to your universities the means of supporting fully and generously those men of genius who have shown themselves capable of extending the boundaries of human knowledge, and demand of them, only, that they devote their lives to study and research, and let me assure you that no money can be spent which will yield a larger or more valuable return.

If you do not look beyond your material interests, the higher life of the nation, which you will thus serve to cherish and foster, will guard your honor and protect your home; and, on the other hand, what can you expect in a nation whose highest ideal is the dollar or what the dollar will buy, but venality, corruption, and ultimate ruin?

But, rising at once to the noblest considerations, and regarding only the welfare of your country and the education of your race, what higher service can you render than by sustaining and cherishing the grandest thought, the purest ideals, and the loftiest aspirations which humanity has reached, and making your universities the altars where the holy fire shall be kept ever burning bright and warm?

Do you think me an enthusiast? Look back through history, and see for yourselves what has made the nations great and glorious. Why is it that, after twenty centuries, the memory of ancient Greece is still enshrined among the most cherished traditions of our race? Is it not because Homer sang, Phidias wrought, and Plato, Aristotle, Demosthenes, Thucydides, with a host of others, thought and wrote? Or, if for you the military exploits of that classic age have the greater charm, do not forget that, were it not for Grecian literature, Thermopylæ, Marathon, and Salamis would have been long since forgotten, and that the bravery, self-devotion, and patriotism which these names embalm were the direct fruits of that higher life which those great thinkers illustrated and sustained.

And, coming down to modern times, what are the shrines in our mother country which we chiefly venerate, and to which the transatlantic pilgrim oftenest di-

rects his steps? Is it her battlefields, her castles and baronial halls, or such spots as Stratford-on-Avon, Abbotsford, and Rydal Mount? Why, then, will we not learn the lesson which history so plainly teaches, and strive for those achievements in knowledge and mental culture which will be remembered with gratitude when all local distinctions and political differences shall have passed away and been forgotten?

While I was considering the line of discourse which I should follow on this occasion, an incident occurred suggesting an historical parallel, which will illustrate, better than any reflections of mine, the truth I would enforce. The ship Faraday arrived on our coast after laying over the bed of the Atlantic another of those electric nerves through which pulsate the thoughts of two continents, and as I read the description of that noble ship, fitted out with all the appliances which modern science had created to insure the successful accomplishment of the enterprise, I remembered that not a century had elapsed since the first obscure phenomena were observed, whose conscientious study, pursued with the unselfish spirit of the scientific investigator, had led to these momentous results, and my imagination carried me back to an autumn day of the year 1786, in the old city of Bologna, in Italy, and I seemed to assist at the memorable experiment which has associated the name of Aloy-

sius Galvani with that mode of electrical energy which flashes through the wire cords that now unite the four quarters of the globe.

Galvani is Professor of Anatomy in the University of Bologna, and there is hanging from the iron balcony of his house a small animal preparation, which is not an unfamiliar sight in Southern Europe, where it is regarded as a delicacy of the table. It is the hind-legs of a frog, from which the skin had been removed, and the great nerve of the back exposed. Six years before, his attention had been called to the fact that the muscles of the frog were convulsed by the indirect action of an electrical machine, under conditions which he had found very difficult to interpret. He had connected the phenomenon with a theory of his own: that electricity—that is, common friction electricity, the only mode of electrical action then known—was the medium of all nervous action; and this had led him into a protracted investigation of the subject, during which he had varied the original experiment in a thousand ways, and he had now suspended the frog's legs to the iron balcony, in order to discover if atmospheric electricity would have any effect on the muscles of the animal.

Galvani has spent a long day in fruitless watching, when, while holding in his hand a brass wire, connected with the muscles of the frog, he rubs the end, apparently

listlessly, against the iron railing, when, lo! the frog's legs are convulsed.

The patient waiting had been rewarded, for this observation was the beginning of a line of discovery which was ere long to revolutionize the world. But Galvani was not destined to follow far the new path he had thus opened. The remarkable fact observed was this: The convulsions of the frog's legs could be produced without the intervention of electricity, or, at least, of the one kind of electricity then known, and Galvani soon found out that the only condition necessary to produce the result was, that the nerve of the frog should be connected with the muscle of the leg by some good electrical conductor.

But, although Galvani followed up this observation with the greatest zeal, and showed remarkable sagacity throughout his whole investigation, yet he was too strongly wedded to his own theory to interpret correctly the facts he observed. He supposed, to the end of his life, that the whole effect was caused by animal electricity flowing through the conductor from the nerve to the muscle, and his experiments were chiefly interesting to himself and to his contemporaries from the light they were supposed to throw on the mysterious principle of life. We now know that animal electricity played only a small part in the phenomena he observed, and that the

chief effects were due to a cause of which he was wholly ignorant.

Galvani published his observations in 1791, in a monograph entitled "The Action of Electricity in Muscular Motion." This publication excited the most marked attention, and, within a year, all Europe was experimenting on frogs' legs. The phenomena were everywhere reproduced, but Galvani's explanation of the phenomena was by no means so universally accepted. His theory was controverted in many quarters, and by no one more successfully than by Alexander Volta, Professor of Physics in the neighboring University of Pavia.

Volta, while admitting, with Galvani, that the muscular contractions were caused by electricity, explained the origin of the electricity in a wholly different way. According to Volta, the electricity originated not in the animal, but in the contact of the dissimilar metals or other materials used in the experiment. This difference of opinion led to one of the most remarkable controversies in the history of science, and for six years, until his death in 1798, Galvani was occupied in defending his theory of animal electricity against the assaults of his distinguished countryman.

This discussion created the liveliest interest throughout Europe. Every scholar of science took sides with one or the other of these eminent Italian philosophers,

and the scientific world became divided into the school of Galvani and the school of Volta. Yet, so far at least as the fundamental experiment was concerned, both were wrong. The electricity came neither from the body of the frog nor from the contact of dissimilar kinds of matter, but was the result of chemical action, which both had equally overlooked.

But, nevertheless, the controversy led to the most important results: for Volta, while endeavoring to sustain his false theory by experimental proofs, was led to the discovery of the Voltaic pile, or, as we now call it, the Voltaic battery, an instrument whose influence on civilization can be compared only with the printing-press and the steam-engine. Yet, although the whole action of the battery was in direct contradiction to his pet theory, still, to the last, Volta persistently defended the erroneous doctrine he had espoused in his controversy with Galvani thirty years before, and he died in 1827, without realizing how great a boon he had been instrumental in conferring on mankind; so true it is that Providence works out his bright designs even through the blindness and mistakes of man.

But there is another lesson to be learned from this history, which can not be too often rehearsed in this self-sufficient age, which boasts so proudly of its practical wisdom. There were, doubtless, many practical men in

that city of Bologna to smile at their sage professor, who had spent ten long years in studying, to little apparent purpose, the twitchings of frogs' hind-legs, and there was many a jest among the courtiers of Europe at the expense of the learned philosophers who "wasted" so much time in discussing the cause of such trivial phenomena. But how is it now?

Less than a century has passed since Galvani's death, and in a small hut on the shores of Valentia Bay may be seen one of the most skillful of a new class of practical men, representing a profession which owes its origin to Galvani and Volta. The *electrician* is watching a spot of light on the scale of an instrument which is called a *galvanometer*. Since the fathers fell asleep, the field of knowledge which they first entered has spread out wider and wider before the untiring explorers who have succeeded them. Oersted and Seebeck, Arago and Ampère, Faraday and our own Henry, have made wonderful discoveries in that field; and other great men, like Steinheil, Wheatstone, Morse, and Thomson, have invented ingenious instruments and appliances, by which these discoveries might be made to yield great practical results.

The spot of light, which the electrician is watching, is reflected from one of the latest of these inventions, the reflecting galvanometer of Thomson. He and his assist-

ants had been watching by turns the same spot for several days, since the Great Eastern had steamed from the bay, paying out a cable of insulated wire. These electricians had no anxiety as to the result, for daily signals had been exchanged between the ship and the shore, as hundreds after hundreds of miles of this electrical conductor had been laid on the bed of the broad ocean. The coast of Newfoundland had already been reached, and they were only waiting for the landing of the cable at the now far-distant end.

At length the light quivers, and the spot begins to move. It answers to concerted signals. And soon the operator spells out the joyful message. The ocean has been spanned with an electric nerve, and the New World responds to the greetings of the Old.

Here is something practical, which all can appreciate, and all are ready to honor. We honor the courage which conceived, the skill which executed, and, above all, the success which crowned the undertaking. But do we not forget that professor of Bologna, with his frogs' legs, who sowed the seed from which all this has sprung? He labored without hope of temporal reward, stimulated by the pure love of truth, and the grain which he planted has brought forth this abundant harvest. Do we not forget, also, that succession of equally noble men, Volta, and Oersted, and Faraday, with many other not less de-

voted investigators of electrical science, without whose unselfish labors the great result never could have been achieved? Such men, of course, need no recognition at our hands, and I ask the question not for their sakes, but for ours. The intellectual elevation of the lives they led was their all-sufficient reward.

It is, however, of the utmost importance for us, citizens of a country with almost unlimited resources, that we should recognize what are the real springs of true national greatness and enduring influence. In this age of material interests, the hand is too ready to say to the head, "I have no need of thee"; and, amid the ephemeral applause which follows the realization of some triumph over matter, we are apt to be deceived, and not observe whence the power came. We associate the great invention with some man of affairs man who overcame the last material obstacle, and who, although worthy of all praise, probably added very little to the total wealth of knowledge of which the invention was an immediate consequence; and, not seeing the antecedents, we are apt to underrate the part which the student or scientific investigator may have contributed to the result.

It is idle, for example, to speak of the electric telegraph as invented by any single man. It was a growth of time, and many of the men who contributed to win this great victory of mind over space "builded far better

than they knew." As I view the subject, that invention is as much a gift of Providence as if the details had been supernaturally revealed. But, whatever may be our speculative views, it is of the utmost importance to the welfare of our community that we should realize the fact that purely theoretical scientific study, pursued for truth's sake, is the essential prerequisite for such inventions. Knowledge is the condition of invention. The old Latin word *invenio* signifies *to meet with*, as well as *to find*, and these great gifts of God are *met with* along the pathway of civilization; but the throng of the world passes them unnoticed, for only those can recognize the treasure whose minds have been stored with the knowledge which the scholar has discovered and made known.

If, then, as no one will deny, science and scholarship are the powers by which improvements in the useful arts are made, I might appeal to your self-interest to support and cherish them. But I should despise myself for appealing to such a motive, and you for requiring it. The supreme importance of science and scholarship to a nation does not depend in the least on the circumstance that important practical results may follow. When, as in the case of Galvani's frogs, they come in the order of Providence, let us thank God for them as a gift which we had no right either to expect or demand. Science, if studied successfully, must be studied for the pure love of

truth; and, if we serve her solely for mercenary ends, her truths, the only gold she offers, will turn to dross in our hands, and we shall degrade ourselves in proportion as we dishonor her.

Galvani, and Volta, and Oersted, who discovered the truths of which the electric telegraph is a simple application, sure to be made as soon as the time was ripe, are not the less to be honored because they died before the fullness of that time had come. We honor them for the truths they discovered, and the lustre of their consecrated lives could be neither enhanced nor impaired by subsequent events; and it is because I am persuaded that such lives are the salt of the world, the saviours of society, that I would lead you to cherish and sustain them; and, that I may enforce this conclusion, allow me to ask your attention to another historical incident, which presents a striking parallelism to the last.

I must take you back to a period which we, of a nation born but yesterday, regard as distant, but which was one of the most noted epochs of modern history—the age of Luther and the Reformation. I must ask you to accompany me to the small town of Allenstein, near Frauenberg, in Eastern Prussia, where, on May 23, 1543, there lay dying one of the great benefactors of mankind.

This man, old at seventy years, "bent and furrowed with labor, but in whose eye the fire of genius was still

glowing," was then known as one of the most learned men of his time. Doctor of medicine as well as of theology, Canon of Frauenberg, Honorary Professor of Bologna and Rome, while devoting his leisure to study, he had passed a life of active benevolence in administering to the bodily as well as the spiritual wants of the ignorant people among whom his lot had been cast. He was also a great mechanical genius, and, by various labor-saving machines, of his own invention, he had contributed greatly to the welfare of the surrounding country.

But the superstitious peasants, although they had hitherto reverenced the great man as their best friend and benefactor, had been recently incited by his enemies and rivals in the church to curse him as a heretic and a wizard. A few days back he had been the unwilling witness of one of those out-of-door spectacles, so common at that time, in which his scientific opinions had been travestied, his charities ridiculed, and his devoted life made the object of slander and reproach. This ingratitude of his flock had broken his heart, and he could not recover from the blow.

The occasion of this outburst of fanaticism was the approaching publication of a work in which he had dared to question the received opinions of theologians and schoolmen, in regard to cosmogony. He had, forsooth, denied that the visible firmament was a solid azure-colored shell, to which the sun and planets were fastened,

and through whose opened doors the rain descended. He had proved that the sun was the center of the system, around which the earth and planets revolved, and, with his clear scientific vision, he had been able to gain glimpses, at least, of the grand conceptions of modern astronomy: For this man was Nicolas Copernicus, and the expected book was his great work—"De Orbium Cœlestium Revolutionibus"—destined to form the broad basis of astronomical science.

The work was printing at Nuremberg, and the last proofs had been returned; but reports had come that a similar outburst of fanaticism was raging at that place, that a mob had burned the manuscript on the public square, and had threatened to break the press should the printing proceed. But, thanks to God! the old man was not to die before the hour of triumph came. While still conscious, a horse, covered with foam, gallops to the door of his humble dwelling, and an armed messenger enters the chamber, who, breathless with haste, places in the hands of the dying man a volume still wet from the press. He has only strength to return a smile of recognition, and murmur the last words:

"Nunc dimittis servum tuum, Domine."

Grand close of a noble life! The seed has been sown—what could we desire more?

Again the centuries roll on—not one, but three—while the seed grows to a great tree, which overshadows the nations. Great minds have never been wanting to cherish and prune it, like Tycho Brahe and Kepler, Galileo and Newton, Laplace and Lagrange; and although at times some, while lingering in the deep shade of the foliage, may have lost sight of the summit, the noble tree has ever pointed upward to direct aspiration and encourage hope.

On the evening of the 24th of September, 1846, in the Observatory of Berlin, a trained astronomical observer was carefully measuring the position of a faint star in the constellation Capricorn. Only the day before, he had received from Le Verrier a letter announcing the result of that remarkable investigation which has made the name of this distinguished French astronomer so justly celebrated. By the studies of the great men who succeeded Copernicus, his system had become so perfected as to enable the astronomer to predict, with unerring certainty, the paths of the planets through the heavens. But there was one failing case. The planet Uranus, then supposed to be the outer planet of the solar system, wandered from the path which theory assigned to it; and although the deviations were but small, yet any discrepancy between theory and observation in so accurate a science as astronomy could not be overlooked.

Long before this, the hypothesis had been advanced that the deviations were caused by the attractive force of an unseen and still more distant planet; but, as no such planet had been discovered, the hypothesis had remained until now wholly barren. The hypothesis, however, was reasonable, and furnished the only conceivable explanation of the facts; and, moreover, if true, the received system of astronomy ought to be able to assign the position and magnitude of the disturbing body, the magnitude and direction of the displacements being given.

This possibility was generally appreciated by astronomers, and the very great length and difficulty of the mathematical calculation which the investigation involved was probably the reason that no one had hitherto undertaken it. Le Verrier, however, had both the courage and the youthful strength required for the work. And now the great work had been done; and, on the 18th of September, Le Verrier had sent to the Observatory of Berlin his communication announcing the final result, namely, that the planet would be found about 5° to the east of the star Delta of Capricorn.

The letter containing this announcement was received by Galle, at Berlin, on the 23d, and it was Galle whom we left measuring the position of that faint star on the evening of the 24th. It so happened that a chart of that

portion of the heavens had recently been prepared by the Berlin Observatory, and was on the eve of publication; and, on the very evening he received the letter, Galle had found, near the position assigned by Le Verrier, a faint star, which was not marked on this chart. The object differed in appearance from the surrounding stars, but still it was perfectly possible that it might be a fixed star which had escaped previous observation.

But, if a fixed star, its position in the constellation would not vary, while, if a planet, a single night would show a perceptible change of place. Hence, you may conceive of the interest with which Galle was measuring anew its position on the evening of the 24th.

The star had moved, and in the direction which theory indicated; and for once, at least, the world rang with applause at a brilliant scientific conquest from which there was not one cent of money to be made. Yet, was that conquest any less important to the world? What had it secured? It had confirmed the theory of astronomy which Copernicus and his successors had built up, and it had clinched the last nail in the proof that those grand conceptions of modern astronomy, now household thoughts, are realities, and not dreams. Certainly no military conquest can compare with this.

Do not smile at the enthusiasm which rates so high a purely intellectual achievement? Go out with me

under the heavens, in some starlight night, and, looking up into the depths of space, recall the truths you have learned in regard to that immensity, and allow the imagination free scope as it stretches out into the infinitudes of time, space, and power, carrying the mind on, bound by bound, through the limitless expanse, until even the imagination refuses to follow, and fairly quails before the mighty form of the Infinite, which rises to confront it! Remember now that your forefathers, of only a few centuries back, saw there nothing but a solid dome hemming in the earth and skies, and that you are able to look upon this grand spectacle only because great minds have lived who have opened your intellectual eyes; and then answer me, is not this result worth all the labor, all the sacrifice, all the treasure it has cost?

Every educated man, who has not sold his birthright for a mess of pottage, lives a grander and nobler life, because the great astronomers have thought and taught, and this elevation of human life is the greatest achievement of which man can boast. Before it all material conquests appear of little worth, and the lustre of all military or civil glory grows dim. Cherish this intellectual life; foster it; sustain it; do what you can by your own spirit and influence, and, if you are blessed with riches, give of your abundance to support and encourage those who, by genius, talent, and devotion, will

widen the intellectual kingdom. Be assured you will thus help to confer an inestimable boon on your race and on your country; and the influence for good will not be felt by the intellectual life of the nation only. That corruption which is now festering at the heart of our body politic, and threatening its destruction, can in no way be fought and conquered so effectually as by keeping constantly before the nation noble and high ideals; for, where the higher life is cherished and honored, the mercenary and sensual motives of action, which both invite and shield corruption, lose much of their force and power.

But you may tell me that there is a life higher than the intellectual life, and that I have ascribed to science and scholarship influences which come only from a source which I have forgotten, or left out of view. My friends, all truth is one and inseparable, and I have therefore made no distinction in this address between the truths of science and truths of religion. The grand old word knowledge, as I have used it, includes both, and, in just the proportion that you reverence religion, you must reverence also true science. All truth is God's truth, and, in praying for the coming of his kingdom, you certainly do not expect that Nature will be divorced from Grace. If the truths of religion required a special revelation, it must be expected that they would transcend human in-

telligence. These very conditions imply conflict, but the conflict comes not from the knowledge, but from the ignorance and conceit of men; and the only proper attitude for the devout scholar is "to labor and to wait." And what more wonderful confirmation could we have of the essential unity of the two phases of truth than is to be found in the fact that the characteristic of science, which I have been endeavoring to illustrate in this address, is the great prominent feature of Christianity? Christianity was revealed in a life, and ever abides a life in the soul of man, to purify, ennoble, and redeem humanity.

"And so the Word had breath, and wrought,
With human hands, the creed of creeds,
In loveliness of perfect deeds,
More strong than all poetic thought—

'Which he may read that binds the sheaf,
Or builds the house, or digs the grave,
And those wild eyes that watch the wave,
In roarings round the coral reef."

III.

THE ELEMENTARY TEACHING OF PHYSICAL SCIENCE.

An Address to the Schoolmasters of Boston, delivered February 4, 1878.

I FELT a great reluctance at accepting the invitation of your excellent superintendent to address you on this occasion; for, although I could claim an unusually long experience in presenting the elements of physical science to college students, I was fully conscious that I knew little of the conditions under which such subjects must be studied, if at all, in the elementary schools, and was therefore in danger of appearing in a capacity which I should most sedulously shun, that of a babbler about impracticable theories of education. It is very easy to criticize another man's labor, and such criticisms, however plausible, do the grossest injustice when, as is often the case, they leave out of view the necessary conditions

and limitations under which the work must be done. While, however, I felt most keenly my incapacity to deal with many of the practical problems which you have to solve, yet, on consideration, I concluded that it was my duty under the circumstances to state as clearly and forcibly as I could the very definite opinions which I had formed on the subject you are discussing, knowing that you will only give such weight to these opinions as your mature judgment can allow. In stating the results of my experience, I can not avoid a certain personal element, which would be wholly inexcusable were it not that the facts, as I think you will admit, form the basis of my argument.

I am a Boston boy, born in this immediate neighborhood, and fitted for college at the "Latin School." It so happened that, while I was very unsuccessfully endeavoring to commit to memory, in the old school-house on School Street, Andrews and Stoddard's Latin grammar, not one word of which I could understand, the "Lowell Institute" lectures were opened at the "Odeon" on Congress Street. At those lectures I got my first taste of real knowledge, and that taste awakened an appetite which has never yet been satisfied. As a boy, I eagerly sought the small amount of popular science which the English literature of that day afforded; and I can now distinctly recall almost every page of Mrs. Marcet's "Con-

versations on Chemistry," which was the first book on my science that I ever read. More to the point than this, a boy's pertinacity, favored by a kind father's indulgence, found the means of repeating, in a small way, most of the experiments first seen at the Lowell Institute lecture; and thus it came to pass that, before I entered college, I had acquired a real, available knowledge of the facts of chemistry; although, with much labor and intense weariness, I had gained only a formal knowledge of those subjects which were then regarded as the only essential preparation for the college course. In college, my attention was almost exclusively devoted to other studies—for, in my day at Cambridge, chemistry was one of the lost arts. But when, the year after I graduated, I was most unexpectedly called upon to give my first course of lectures, the only laboratory in which I had worked was the shed of my father's house on Winthrop Place, and the only apparatus at my command was what this boy's laboratory contained. With these simple tools, or, as I should rather say, because they were so simple, I gained that measure of success which determined my subsequent career.

I feel that I owe you a constant apology for these personal details, and I should not be guilty of them did I not believe that they establish two points more conclusively than I could prove them in any other way. First,

that it is perfectly possible for a child before fifteen years of age to acquire a real and living knowledge of the fundamental facts of nature on which physical science is based. Secondly, that this knowledge can be effectually gained by the use of the simplest tools. Let me add that this is not a question of natural endowments or special aptitudes, for every one who has studied from the love of knowledge has had the same experience; and I do not believe that, if my first taste of real knowledge had been of history, nay, I will even say, of philology, instead of chemistry, the circumstance would have materially influenced my success in life, however different the direction into which it might have turned my study. My early tastes were utterly at variance with all my surroundings and all my inheritances, and were simply determined by the accident which first satisfied that natural thirst for knowledge which every child experiences to a greater or less degree—a desire most rudely repressed in our usual methods of teaching.

My bitter experience as a pupil in the Boston Latin School and my subsequent more fortunate experience of thirty years as a teacher in Harvard College have impressed me most profoundly with the conviction that the only way to arouse and sustain a love for knowledge in children is to cultivate their perceptive faculties. To present the rudiments of knowledge to immature minds

in an abstract form, whether the subject be grammar or physical science, is, in my judgment, not only culpable folly, but also downright wrong. And, if, to those who have been accustomed to the long established routine of our public school, my opinions may appear revolutionary and extreme, I am, nevertheless, sure that they would receive the universal assent of the men whom all would recognize as the foremost scientific teachers of the world. I can well remember that when, many years ago, the late Professor Agassiz declared in my hearing that he would have no text-books used in his museum, I thought his plan of pure object-teaching chimerical in the extreme, and yet experience has not only convinced me of the wisdom of his judgment in regard to the teaching of natural history, but brought me to a similar conclusion in regard to the elementary teaching both of natural philosophy and of chemistry.

Allow me then to express my firm persuasion that it is not only useless but injurious to the education of young minds to present to them at the outset any department of physical science as a body of definitions, principles, laws, or theories; and that in elementary schools only such facts should be taught as can be verified by the experience of the pupil, or by such simple experiments as the pupils can try for themselves. The usual method of committing by heart the words of a

school-book, and repeating them at the dictation of a teacher, may afford a good exercise for the memory, but it is absurd to regard such a task as a lesson in physical science, and this kind of study can be spent with vastly greater profit on the spelling-book.

There is one department of physical science which has been taught in this absurd way in our schools from time immemorial. I refer, of course, to the study of geography, and I leave for you to judge whether the result is worth the one hundredth part of the toil and drudgery spent in obtaining it. Let us suppose that your child is able to give you the names of all the rivers, bays, and capes from Greenland to Patagonia, how much more does that child know of the structure and social relations of this globe on which its lot has been cast than it did before this senseless feat was attempted, a feat, moreover, to which only a child's memory would be equal? And, when you turn to your own experience, what is the outcome of all the time and labor spent on geography? Is it not solely just that portion of your knowledge which, in spite of the system, was direct object-teaching—the images you insensibly acquired from the maps and pictures in the school-books?

But there is a very different way of teaching geography, by which the study may be made a pleasure, not a task. The teacher does not begin with abstract defini-

tions of rivers, and bays, and oceans, which convey no definite meaning to a child, but with Charles River, Boston Harbor, and the Atlantic Ocean, which are to him real things, however imperfect his conceptions of their extent. The child is first shown, not a map of the globe, which he can not by any possibility understand, but a map of a very limited region around his own home. He is taught how to find the north and south, the east and west directions. He is encouraged to make excursions to verify the map, or to add to its details, and such excursions may be made to have for him all the zest of voyages of discovery; and when thus the rudiments of geographical science have been mastered, not in technical terms, but in substance, then the teacher may begin to expand the horizon of the pupil's knowledge, judiciously omitting details in proportion as distance increases, until at length the general survey embraces the globe. Of course, such teaching as this can only be given orally with the help of proper apparatus, such as wall maps, and globes, and photographs. It must take the interrogative form, and the questions should be directed to bring out the child's already acquired knowledge, and to lead him to observe facts which had hitherto escaped his notice. What a child reads in a book, or even what you tell him, is never one half learnt, unless his interest is aroused. But what a child observes for himself he never

forgets, and when you have thus aroused his interest you can associate a large number of facts with one observation, and these all crystallize in his memory around this nucleus.

This is no mere theory, no untried method which I am advocating. So far from it, I am describing the precise method which has been used for many years in Germany, where the science of education is far better understood than with us, and where economy both of time and labor in teaching is most carefully studied. If our school committees could attend and understand a single exercise in geography, such as are daily given in the elementary schools of Prussia, I am sure that at least one form of child torture would soon disappear from the primary schools of this country. Indeed, I already see evidence of a growing public opinion on this subject, an effect which I trace in no small measure to the influence of the Department of Education of the Exhibition at Philadelphia in 1876.

That which is true of geography applies with still greater force to such subjects as physics and chemistry, since the abstract conceptions which these sciences involve are more abstruse, and the language by which the conceptions are expressed or defined far less plain than is the case with the older and more descriptive branch of knowledge. Hence, as sciences, properly so called,

that is, as philosophical systems, they have no place whatever in elementary education. But, underlying these systems, there is a great multitude of phenomena which a child can be led to observe and apprehend as readily as the facts of geography. Take that subject—mechanics—which our ordinary school-books very philosophically but most unpractically place at the beginning of what they call "Natural" Philosophy. How many of the fundamental facts of this difficult subject can be made familiar to a child? Select, as an example, Newton's "First Law of Motion." Suppose you make a boy memorize the ordinary rule, "Every body continues in a state of rest or of uniform motion in a straight line until acted upon by some external force," how much will he know about it? Suppose you make him do a lot of problems involving distances, velocities, and times, will he know any more about it? But ask him, "Can you pitch a ball as well as your playmate?" and he answers at once, "No; John is stronger than I am." And then, if again you ask, "Can you catch John's ball?" he will probably reply, "Of course, not! It requires a boy as strong as John to catch his balls." And thus, by a few well-directed questions, you would bring that boy to learn a lesson which he would never forget, and which he would recall every time he played base-ball; namely, that John's swift balls could not be set in motion without an expenditure

of a definite amount of muscular effort, and could not be stopped without the exertion of an equal amount of what, after a while, you could get him to call *force*. From the ball you would naturally pass to the railroad train or the steamboat, and I should not wonder if, with a little patience, you could bring even a boy to understand that motion can not be maintained against a resistance, in other words, that work can not be done without a constant expenditure of muscular effort, or of some other source of power; and it is a fond hope of mine that by the time these boys grow into men our intelligent New England community might become so far educated in the elementary principles of mechanics that no self-sustained motors, nor other mechanical nostrums which claim to have superseded the primeval curse—if that law was a curse, which compels man to earn his bread with the sweat of his brow—will receive the sanction of our respectable journals; and then—if they have not previously learned the lesson by dire experience—we may hope to persuade our people of the parallel and equally elementary principle of political economy, that value can not be legislated into rags.

But, my friends, our subject gives no occasion for banter, and presents aspects too serious to be treated lightly or in jests. As inhabitants of a not over-fruitful land, and, therefore, members of a community which

must excel, if at all, solely by its enterprise and intelligence, we have a duty to our children which we can not avoid, if we would, and for which we shall be held responsible by our posterity. These children are entering life surrounded not only by all the wonders and glories of nature, but, also, by giant conditions, which, whether stationed on their path as a blessing or a curse, will inevitably strike if their behests are not obeyed. So far as science has been able to define these giant forms, it is our duty, as it is our privilege, to point them out to those we are bound to protect and guide; and in many cases it is in our power to change the curse into a blessing, and to transform the destructive demon into a guardian angel. After that command of language which the necessities of civilized life imperatively require, there is no acquisition which we can give our children that will exert so important an influence on their material welfare as a knowledge of the laws of nature, under which they must live and to which they must conform; and throughout whose universal dominion the only question is whether men shall grovel as ignorant slaves or shall rule as intelligent servants. Yes; rule by obeying. "Ich Dien"; for only under that motto, which, five hundred years ago, the great Black Prince bore so victoriously through the fields of Cressy and Poitiers, can man ever rule in Nature's kingdom.

I regard it, therefore, as the highest duty and the most enlightened self-interest of a community like this to provide the best means for the instruction of its children in the elements of physical science; and I was, therefore, most anxious to do all in my power to second the enlightened efforts of your eminent Superintendent in this direction. You must remember, however, that the best tools are worthless in themselves, and can secure no valuable results unless judiciously used. Indeed, there is danger in too many tools, and I have a great horror of that array of brass-work which is usually miscalled "philosophical" apparatus. The greater part of this is, in my opinion, a mere hindrance to the teacher, because it at once erects a barrier between the scholar and the simple facts of nature, and the child inevitably associates with the phenomenon illustrated some legerdemain, and looks on your experiments very much as he would on the exhibition of a Houdin or a Signor Blitz. The secret of success in teaching physical science is to use the simplest and most familiar means to illustrate your point.

When a very young man I was favored with an introduction to Michael Faraday, and had the privilege of attending a portion of a course of lectures which this noble man was then in the habit of giving every Christmas season to a juvenile auditory at the Royal Institution of London. As a boy, I had become familiar with lec-

tures on chemistry at the Lowell Institute, where they did not lack the pomp of circumstance or the display of apparatus, and I had come to associate these elements with the conditions of success in lectures of this kind. What, then, was my surprise to find Faraday, the acknowledged leader of the world in his science, and who had every means of illustration at his command, using the plainest language and the simplest tools. When, in my youthful admiration at the result, I expressed, after one of the lectures, my surprise at the simplicity of the means employed, the great master replied: "That is the whole secret of interesting these young people. I always use the simplest means, but I never leave a point not illustrated. If I mention the force of gravitation I take up a stone and let it drop." At this distance of time, I can not be sure that I quote his exact language, but the lesson and the illustration I could not forget; and to this lesson, more than to any other one thing, I owe whatever success I have had as a teacher of physical science.

I repeat, therefore, it is not only useless but injurious in the education of young minds to present any department of physical science as a body of definitions, principles, laws, or theories; and that in elementary schools such facts only should be taught as can be verified either by the experience of the pupils or by the simplest experiments, which the pupils can repeat by

themselves; and now, after this discussion, I add, that the teacher must depend on his own ingenuity for his experiments, and on his intercourse with his pupils for his instruction.

But you will tell me all this involves grave difficulties, and conditions incompatible with our ordinary school life. I freely admit the difficulties, but I am none the less sure that, unless science can be taught on the principles I have endeavored to illustrate, it had better not be taught at all. I know very well that the proper teaching of physical science is wholly incompatible with our usual school methods. But this only proves to me that these methods ought to be changed, and I am persuaded that the changes required will benefit the literary and classical as well as the scientific courses of study. For do not the same general principles apply to the acquisition of knowledge in all subjects? And when a child's perceptive faculties have been duly stimulated, and his intelligence fully awakened, he will find interest in grammar, in literature, or in history, as well as in science.

In repelling the reproach of narrowness, to which our elective system at Cambridge undoubtedly frequently leads, how often have I urged the self-evident proposition that to arouse a love of study in any subject, I care not how subordinate its importance or how limited its scope, is to take the first step toward making your man a scholar;

while to fail to gain his interest in any study is to lose the whole end of education—and what is true of the man is still more true of the child. Classical culture on the one hand and scientific culture on the other are excellent things, but, if your boy can not be made to take an interest either in classics or in science, how plain it is that such treasures are not for him, and, in the absence of the one condition which can give value to any study, how idle and inconsequent all questions in regard to the relative merits of these studies appear! On the other hand, a love of study once gained, all studies are alike good.

And as with the pupil, so with the teacher. No teaching is of any real value that does not come directly from the intelligence and heart of the teacher, and thus appeals to the intelligence and heart of the pupil. It, of course, implies more acquisition, and it requires far more energy to teach from one's own knowledge than to teach from a book, but then, just in proportion to the difficulties overcome, does the teacher raise his profession and ennoble himself. There is no nobler service than the life of a true teacher; but the mere task-master has no right to the teacher's name, and can never attain the teacher's reward.

IV.

THE RADIOMETER:

A FRESH EVIDENCE OF A MOLECULAR UNIVERSE.

A Lecture delivered in the Sanders Theatre of Harvard University, March 6, 1878.

No one who is not familiar with the history of physical science can appreciate how very modern are those grand conceptions which add so much to the loftiness of scientific studies; and, of the many who, on one of our starlit nights, look up into the depths of space, and are awed by the thoughts of that immensity which come crowding upon the mind, there are few, I imagine, who realize the fact that almost all the knowledge which gives such great sublimity to that sight is the result of comparatively recent scientific investigation; and that the most elementary student can now gain conceptions of the immensity of the universe of which the fathers of astronomy never dreamed. And how very grand are

the familiar astronomical facts which the sight of the starry heavens suggests!

Those brilliant points are all suns like the one which forms the center of our system, and around which our earth revolves; yet so inconceivably remote, that, although moving through space with an incredible velocity, they have not materially changed their relative position since recorded observations began. Compared with their distance, the distance of our own sun—92,000,000 miles—seems as nothing; yet how inconceivable even that distance is when we endeavor to mete it out with our terrestrial standards! For if, when Copernicus—the great father of modern astronomy—died, in 1543, just at the close of the Protestant Reformation, a messenger had started for the sun, and traveled ever since with the velocity of a railroad train—thirty miles an hour—he would not yet have reached his destination!

Evidently, then, no standards, which, like our ordinary measures, bear a simple or at least a conceivable relation to the dimensions of our own bodies, can help us to stretch a line in such a universe. We must seek for some magnitude which is commensurate with these immensities of space; and, in the wonderfully rapid motion of light, astronomy furnishes us with a suitable standard. By the eclipses of Jupiter's satellites the astronomers have determined that this mysterious effluence reaches us

from the sun in eight minutes and a half, and therefore must travel through space with the incredible velocity—shall I dare to name it?—of 186,000 miles in a second of time! Yet, inconceivably rapid as this motion is, capable of girdling the earth nearly eight times in a single second, the very nearest of the fixed stars, *a* Centauri, is so remote that the light by which it will be seen in the southern heavens to-night, near that magnificent constellation, the Southern Cross, must have started on its journey three years and a half ago. But this light comes from merely the threshold of the stellar universe; and the telescope reveals to us stars so distant that, had they been blotted out of existence when history began, the tidings of the event could not yet have reached the earth!

Compare now with these grand conceptions the popular belief of only a few centuries back. Where we look into the infinite depths, our Puritan forefathers saw only a solid dome hemming in the earth and skies, and through whose opened doors the rain descended. They regarded the sun and moon merely as great luminaries set in this firmament to rule the day and night, and to their understandings the stars served no better purpose than the spangles which glitter on the azure ceiling of many a modern church. The great work of Copernicus, "De Orbium Cœlestium Revolutionibus," which was destined, ultimately, to overthrow the crude cosmography

which Christianity had inherited from Judaism, was not published until just at the close of the author's life in 1543, the date before mentioned. The telescope, which was required to fully convince the world of its previous error, was not invented until more than half a century later, and it was not until 1835 that Struve detected the parallax of *a* Lyræ. The measurement of this parallax, together with Bessel's determination of the parallax of 61 Cygni, and Henderson's that of *a* Centauri, at about the same time, gave us our first accurate knowledge of the distances of the fixed stars.

To the thought I have endeavored to express, I must add another, before I can draw the lesson which I wish to teach. Great scientific truths become popularized very slowly, and, after they have been thoroughly worked out by the investigators, it is often many years before they become a part of the current knowledge of mankind. It was fully a century after Copernicus died, with his great volume—still wet from the press of Nuremberg—in his hands, before the Copernican theory was generally accepted even by the learned; and the intolerant spirit with which this work was received and the persecution which Galileo encountered more than half a century later were due solely to the circumstance that the new theory tended to subvert the popular faith in the cosmography of the Church. In modern times,

with the many popular expositors of science, the diffusion of new truth is more rapid; but even now there is always a long interval after any great discovery in abstract science before the new conception is translated into the language of common life, so that it can be apprehended by the mass even of educated men.

I have thus dwelt on what must be familiar facts in the past history of astronomy, because they illustrate and will help you to realize the present condition of a much younger branch of physical science; for, in the transition period I have described, there exists now a conception which opens a vision into the microcosmos beneath us as extensive and as grand as that which the Copernican theory revealed into the macrocosmos above us.

The conception to which I refer will be at once suggested to every scientific scholar by the word *molecule.* This word is a Latin diminutive, which means, primarily, a small mass of matter; and, although heretofore often applied in mechanics to the indefinitely small particles of a body between which the attractive or repulsive forces might be supposed to act, it has only recently acquired the exact significance with which we now use it.

In attempting to discover the original usage of the

word molecule, I was surprised to find that it was apparently first introduced into science by the great French naturalist, Buffon, who employed the term in a very peculiar sense. Buffon does not seem to have been troubled with the problem which so engrosses our modern naturalists—how the vegetable and animal kingdoms were developed into their present condition—but he was greatly exercised by an equally difficult problem, which seems to have been lost sight of in the present controversy, and which is just as obscure to-day as it was in Buffon's time, at the close of the last century, and that is, Why species are so persistent in Nature; why the acorn always grows into the oak, and why every creature always produces of its kind. And, if you will reflect upon it, I am sure you will conclude that this last is by far the more fundamental problem of the two, and one which necessarily includes the first. That, of two eggs, in which no anatomist can discover any structural difference, the one should, in a few short years, *develop* an intelligence like Newton's, while the other soon ends in a Guinea-pig, is certainly a greater mystery than that, in the course of unnumbered ages, monkeys, by insensible gradations, should *grow* into men.

In order to explain the remarkable constancy of species, Buffon advanced a theory which, when freed from a good deal that was fanciful, may be expressed thus:

The attributes of every species, whether of plants or of animals, reside in their ultimate particles, or, to use a more philosophical but less familiar word, *inhere* in these particles, which Buffon names *organic molecules.* According to Buffon, the oak owes all the peculiarities of its organization to the special oak molecules of which it consists; and so all the differences in the vegetable or animal kingdom, from the lowest to the highest species, depend on fundamental peculiarities with which their respective molecules were primarily endowed. There must, of course, be as many kinds of molecules as there are different species of living beings; but, while the molecules of the same species were supposed to be exactly alike, and to have a strong affinity or attraction for each other, those of different species were assumed to be inherently distinct and to have no such affinities. Buffon further assumed that these molecules of organic nature were diffused more or less widely through the atmosphere and through the soil, and that the acorn grew to the oak simply because, consisting itself of oak molecules, it could draw only oak molecules from the surrounding media.

With our present knowledge of the chemical constitution of organic beings, we can find a great deal that is both fantastic and absurd in this theory of Buffon; but it must be remembered that the science of chemistry is

almost wholly a growth of the present century, while Buffon died in 1788; and, if we look at the theory solely from the standpoint of his knowledge, we shall find in it much that was worthy of this great man. Indeed, in our time, the essential features of the theory of Buffon have been transferred from natural history to chemistry almost unchanged.

According to our modern chemistry, the qualities of every substance reside or inhere in its molecules. Take this lump of sugar. It has certain qualities with which every one is familiar. Are those qualities attributes of the lump or of its parts? Certainly of its parts; for, if we break up the lump, the smallest particles will still taste sweet and show all the characteristics of sugar. Could we, then, carry on this subdivision indefinitely, provided only we had senses or tests delicate enough to recognize the qualities of sugar in the resulting particles? To this question, modern chemistry answers decidedly, No! You would before long reach the smallest mass that can have the qualities of sugar. You would have no difficulty in breaking up these masses, but you would then obtain, not smaller particles of sugar, but particles of those utterly different substances which we call carbon, oxygen, and hydrogen—in a word, particles of the elementary substances of which sugar consists. These ultimate particles of sugar we call the molecules of sugar,

and thus we come to the present chemical definition of a molecule, "*The smallest particles of a substance in which its qualities inhere,*" which, as you see, is a reproduction of Buffon's idea, although applied to matter and not to organism.

A lump of sugar, then, has its peculiar qualities because it is an aggregate of molecules which have those qualities, and a lump of salt differs from a lump of sugar simply because the molecules of salt differ from those of sugar, and so with every other substance. There are as many kinds of molecules in Nature as there are different substances, but all the molecules of the same substance are absolutely alike in every respect.

Thus far, as you see, we are merely reviving in a different association the old ideas of Buffon. But just at this point comes in a new conception, which gives far greater grandeur to our modern theory: for we conceive that those smallest particles in which the qualities of a substance inhere are definite bodies or systems of bodies moving in space, and that *a lump of sugar is a universe of moving worlds.*

If on a clear night you direct a telescope to one of the many star-clusters of our northern heavens, you will have presented to the eye as good a diagram as we can at present draw of what we suppose would, under certain circumstances, be seen in a lump of sugar if we could

look into the molecular universe with the same facility with which the telescope penetrates the depths of space.

Do you tell me that the absurdities of Buffon were wisdom when compared with such wild speculations as these? The criticism is simply what I expected, and I must remind you that, as I intimated at the outset, this conception of modern science is in the transition period of which I then spoke, and, although very familiar to scientific scholars, has not yet been grasped by the popular mind. I can further only add that, wild as it may appear, the idea is the growth of legitimate scientific investigation, and express my conviction that it will soon become as much a part of the popular belief as those grand conceptions of astronomy to which I have referred.

Do you rejoin that we can see the suns in a stellar cluster, but can not even begin to see the molecules? I must again remind you that, in fact, you only see points of light in the field of the telescope, and that your knowledge that these points are immensely distant suns is an inference of astronomical science; and, further, that our knowledge—if I may so call our confident belief—that the lump of sugar is an aggregate of moving molecules is an equally legitimate inference of molecular mechanics, a science which, although so much newer, is as positive a field of study as astronomy. Moreover, sight is

not the only avenue to knowledge; and, although our material limitations forbid us to expect that the microscope will ever be able to penetrate the molecular universe, yet we feel assured that we have been able by strictly experimental methods to weigh molecular masses and measure molecular magnitudes with as much accuracy as those of the fixed stars.

Of all forms of matter the gas has the simplest molecular structure, and, as might be anticipated, our knowledge of molecular magnitudes is as yet chiefly confined to materials of this class. I have given below some of the results which have been obtained in regard to the molecular magnitudes of hydrogen gas, one of the best studied of this class of substances; and, although the vast numbers are as inconceivable as are those of astronomy, they can not fail to impress you with the reality of the magnitudes they represent. I take hydrogen gas for my illustration rather than air, because our atmosphere is a mixture of two gases, oxygen and nitrogen, and therefore its condition is less simple than that of a perfectly homogeneous material like hydrogen. The molecular dimensions of other substances, although varying very greatly in their relative values, are of the same order of magnitude as these.*

* As some of the readers of this volume may be interested to compare these values, we reproduce the "Table of Molecular Data" from Professor

Dimensions of Hydrogen Molecules calculated for Temperature of Melting Ice, and for the Mean Height of the Barometer of the Sea Level:

Mean velocity, 6,099 feet a second.

Mean path, 31 ten-millionths of an inch.

Collisions, 17,750 millions each second.

Diameter, 438,000, side by side, measure $\frac{1}{100}$ of an inch.

Mass, 14 (millions ^{3}) weigh $\frac{1}{1000}$ of a grain.

Gas-volume, 311 (millions ^{3}) fill one cubic inch.

To explain how the values here presented were obtained would be out of place in a popular lec-

Clerk Maxwell's lecture on "Molecules," delivered before the British Association at Bradford, and published in "Nature," September 25, 1873.

Molecular Magnitudes at Standard Temperature and Pressure, 0° C. and 76 c. m.

RANK ACCORDING TO ACCURACY OF KNOWLEDGE.	Hydrogen.	Oxygen.	Carbonic Oxide.	Carbonic Dioxide.
RANK I.				
Relative mass	1	16	14	22
Velocity in metres per second	1,859	465	497	396
RANK II.				
Mean path in ten billionths (10^{-10}) of a metre	965	560	482	379
Collisions each second—number of millions	17,750	7,646	9,489	9,720
RANK III.				
Diameter in hundred billionths (10^{-11}) of a metre	58	76	83	93
Mass in ten million million million millionths (10^{-25}) of a gramme	46	736	644	1,012

Number of molecules in one cubic centimetre of every gas is nineteen million million million on 19 (10^{18}).

Two million hydrogen molecules side by side measure a little over one millimetre.

ture,* but a few words in regard to two or three of the data are required to elucidate the subject of this lecture.

First, then, in regard to the mass or weight of the molecules. So far as their relative values are concerned, chemistry gives us the means of determining the molecular weights with very great accuracy; but when we attempt to estimate their weights in fractions of a grain —the smallest of our common standards—we can not expect precision, simply because the magnitudes compared are of such a different order; and the same is true of most of the other absolute dimensions, such as the diameter and volume of the molecules. We only regard the values given in our table as a very rough estimate, but still we have good grounds for believing that they are sufficiently accurate to give us a true idea of the order of the quantities with which we are dealing; and it will be seen that, although the numbers required to express the relations to our ordinary standards are so large, these molecular magnitudes are no more removed from us on the one side than are those of astronomy on the other.

Passing next to the velocity of the molecular motion, we find in that a quantity which, although large, is commensurate with the velocity of sound, the velocity of a

* *See* Professor Maxwell's lecture, *loc. cit.;* also, Appletons' "Cyclopædia," article "Molecules."

rifle-ball, and the velocities of many other motions with which we are familiar. We are, therefore, not comparing, as before, quantities of an utterly different order, and we have confidence that we have been able to determine the value within very narrow limits of error. But how surprising the result is! Those molecules of hydrogen are constantly moving to and fro with this great velocity, and not only are the molecules of all aëriform substances moving at similar, although differing rates, but the same is equally true of the molecules of every substance, whatever may be its state of aggregation.

The gas is the simplest molecular condition of matter, because in this state the molecules are so far separated from each other that their motions are not influenced by mutual attractions. Hence, in accordance with the well-known laws of motion, gas molecules must always move in straight lines and with a constant velocity until they collide with each other or strike against the walls of the containing vessel, when, in consequence of their elasticity, they at once rebound and start on a new path with a new velocity. In these collisions, however, there is no loss of motion, for, as the molecules have the same weight and are perfectly elastic, they simply change velocities, and whatever one may lose the other must gain.

But, if the velocity changes in this way, you may ask,

What meaning has the definite value given in our table? The answer is, that this is the mean value of the velocity of all the molecules in a mass of hydrogen gas under the assumed conditions; and, by the principle just stated, the mean value can not be changed by the collisions of the molecules among themselves, however great may be the change in the motion of the individuals.

In both liquids and solids the molecular motions are undoubtedly as active as in a gas, but they must be greatly influenced by the mutual attractions which hold the particles together, and hence the conditions are far more complicated, and present a problem which we have been able to solve only very imperfectly, and with which, fortunately, we have not at present to deal.

Limiting, then, our study to the molecular condition of a gas, picture to yourselves what must be the condition of our atmosphere, with its molecules flying about in all directions. Conceive what a molecular storm must be raging about us, and how it must beat against our bodies and against every exposed surface. The molecules of our atmosphere move, on an average, nearly four (3·8) times slower than those of hydrogen under the same conditions; but then they weigh, on an average, fourteen and a half times more than hydrogen molecules, and therefore strike with as great energy. And do not think that the effect of these blows is insignificant because the

molecular projectiles are so small; they make up by their number for what they want in size.

Consider, for example, a cubic yard of air, which, if measured at the freezing-point, weighs considerably over two pounds. That cubic yard of material contains over two pounds of molecules, which are moving with an average velocity of 1,605 feet a second, and this motion is equivalent, in every respect, to that of a cannon-ball of equal weight rushing along its path at the same tremendous rate. Of course, this is true of every cubic yard of air at the same temperature; and, if the motion of the molecules of the atmosphere around us could by any means be turned into one and the same direction, the result would be a hurricane sweeping over the earth with this velocity—that is, at the rate of 1,094 miles an hour—whose destructive violence not even the Pyramids could withstand.

Living as we do in the midst of a molecular tornado capable of such effects, our safety lies wholly in the circumstance that the storm beats equally in all directions at the same time, and the force is thus so exactly balanced that we are wholly unconscious of the tumult. Not even the aspen-leaf is stirred, nor the most delicate membrane broken; but let us remove the air from one of the surfaces of such a membrane, and then the power of the molecular storm be-

comes evident, as in the familiar experiments with an air-pump.

As has already been intimated, the values of the velocities both of hydrogen and of air molecules given above were measured at a definite temperature, 32° of our Fahrenheit thermometer, the freezing-point of water; and this introduces a very important point bearing on our subject, namely, that the molecular velocities vary very greatly with the temperature. Indeed, according to our theory, this very molecular motion constitutes that state or condition of matter which we call temperature. A hot body is one whose molecules are moving comparatively rapidly, and a cold body one in which they are moving comparatively slowly. Without, however, entering into further details, which would involve the whole mechanical theory of heat, let me call your attention to a single consequence of the principle I have stated.

When we heat hydrogen, air, or any mass of gas, we simply increase the velocity of its moving molecules. When we cool the gas, we simply lessen the velocity of the same molecules. Take a current of air which enters a room through a furnace. In passing it comes in contact with heated iron, and, as we say, is heated. But, as we view the process, the molecules of the air, while in contact with the hot iron, collide with the very rapidly oscillating metallic molecules, and fly back as a billiard-

ball would under similar circumstances, with a greatly increased velocity, and it is this more rapid motion which alone constitutes the higher temperature.

Consider, next, what must be the effect on the surface. A moment's reflection will show that the normal pressure exerted by the molecular storm, always raging in the atmosphere, is due not only to the impact of the molecules, but also to the reaction caused by their rebound. When the molecules rebound, they are, as it were, driven away from the surface in virtue of the inherent elasticity both of the surface and of the molecules. Now, what takes place when one mass of matter is driven away from another—when a cannon-ball is driven out of a gun, for example? Why, the gun *kicks!* And so every surface from which molecules rebound must *kick;* and, if the velocity is not changed by the collision, one half of the pressure caused by the molecular bombardment is due to the recoil. From a heated surface, as we have said, the molecules rebound with an increased velocity, and hence the recoil must be proportionally increased, determining a greater pressure against the surface.

According to this theory, then, we should expect that the air would press unequally against surfaces at different temperatures, and that, other things being equal, the pressure exerted would be greater the higher the tem-

perature of the surface. Such a result, of course, is wholly contrary to common experience, which tells us that a uniform mass of air presses equally in all directions and against all surfaces of the same area, whatever may be their condition. It would seem, then, at first sight, as if we had here met with a conspicuous case in which our theory fails. But further study will convince us that the result is just what we should expect in a dense atmosphere like that in which we dwell; and, in order that this may become evident, let me next call your attention to another class of molecular magnitudes.

It must seem strange indeed that we should be able to measure molecular velocities; but the next point I have to bring to your notice is stranger yet, for we are confident that we have been able to determine with approximate accuracy for each kind of gas molecule the average number of times one of these little bodies runs against its neighbors in a second, assuming, of course, that the conditions of the gas are given. Knowing, now, the molecular velocity and the number of collisions a second, we can readily calculate the mean path of the molecule—that is, the average distance it moves, under the same conditions, between two successive collisions. Of course, for any one molecule, this path must be constantly varying; since, while at one time the molecule may find a clear coast and make a long run, the very

next time it may hardly start before its course is arrested. Still, taking a mass of gas under constant conditions, the doctrine of averages shows that the mean path must have a definite value, and an illustration will give an idea of the manner in which we have been able to estimate it.

The nauseous, smelling gas we call sulphide of hydrogen has a density only a little greater than that of air, and its molecules must therefore move with very nearly as great velocity as the average air molecule—that is to say, about fourteen hundred and eighty feet a second; and we might therefore expect that, on opening a jar of the gas, its molecules would spread instantly through the surrounding atmosphere. But, so far from this, if the air is quiet, so that the gas is not transported by currents, a very considerable time will elapse before the characteristic odor is perceived on the opposite side of an ordinary room. The reason is obvious: the molecules must elbow their way through the crowd of air molecules which already occupy the space, and can therefore advance only slowly; and it is obvious that, the oftener they come into collision with their neighbors, the slower their progress must be. Knowing, then, the mean velocity of the molecular motion, and being able to measure by appropriate means *the rate of diffusion*, as it is called, we have the data from which we can calculate both the number

of collisions in a second and also the mean path between two successive collisions. The results, as we must expect, are of the same order as the other molecular magnitudes. But, inconceivably short as the free * path of a molecule certainly is, it is still, in the case of hydrogen gas, 136 times the diameter of the moving body, which would certainly be regarded among men as quite ample elbow-room.

Although, in this lecture, I have as yet had no occasion to mention the radiometer, I have by no means forgotten my main subject, and everything which has been said has had a direct bearing on the theory of this remarkable instrument; and still, before you can understand the great interest with which it is regarded, we must follow out another line of thought, converging on the same point.

One of the most remarkable results of modern science is the discovery that all energy at work on the surface of this planet comes from the sun. Most of you probably saw, at our Centennial Exhibition, that great artificial cascade in Machinery Hall, and were impressed with the

* There is an obvious distinction between the free and the disturbed path of a molecule, and we can not overlook in our calculations the perturbations which the collisions necessarily entail. Such considerations greatly complicate the problem, which is far more difficult than would appear from the superficial view of the subject that can alone be given in a popular lecture.

power of the steam-pump which could keep flowing such a mass of water. But, also, when you stood before the falls at Niagara, did you realize the fact that the enormous floods of water which you saw surging over those cliffs were in like manner supplied by an all-powerful pump, and that pump the sun? And not only is this true, but it is equally true that every drop of water that falls, every wave that beats, every wind that blows, every creature that moves on the surface of the earth, one and all, are animated by that mysterious effluence we call the sunbeam. I say mysterious effluence; for how that power is transmitted over those 92,000,000 miles between the earth and the sun is still one of the greatest mysteries of Nature.

In the science of optics, as is well known, the phenomena of light are explained by the assumption that the energy is transmitted in waves through a medium which fills all space called the luminiferous ether, and there is no question that this theory of Nature, known in science as the Undulatory Theory of Light, is, as a working hypothesis, one of the most comprehensive and searching which the human mind has ever framed. It has both correlated known facts and pointed the way to remarkable discoveries. But, the moment we attempt to apply it to the problem before us, it demands conditions which tax even a philosopher's credulity.

As sad experience on the ocean only too frequently teaches, energy can be transmitted by waves as well as in any other way. But every mechanic will tell you that the transmission of energy, whatever be the means employed, implies certain well-known conditions. Assume that the energy is to be used to turn the spindles of a cotton mill. The engineer can tell you just how many horse-power he must supply for every working-day, and it is equally true that a definite amount of energy must come from the sun to do each day's work on the surface of the globe. Further, the engineer will also tell you that, in order to transmit the power from his turbine or his steam-engine, he must have shafts and pulleys and belts of adequate strength, and he knows in every case what is the lowest limit of safety. In like manner, the medium through which the energy which runs the world is transmitted must be strong enough to do the immense work put upon it; and, if the energy is transmitted by waves, this implies that the medium must have an enormously great elasticity, an elasticity vastly greater than that of the best-tempered steel.

But turn now to the astronomers, and learn what they have to tell us in regard to the assumed luminiferous ether through which all this energy is supposed to be transmitted. Our planet is rushing in its orbit around the sun at an average rate of over 1,000 miles a minute,

and makes its annual journey of some 550,000,000 miles in 365 days, 6 hours, 9 seconds, and $\frac{6}{10}$ of a second. Mark the tenths; for astronomical observations are so accurate that, if the length of the year varied permanently by the tenth of a second, we should know it; and you can readily understand that, if there were a medium in space which offered as much resistance to the motion of the earth as would gossamer threads to a race-horse, the planet could never come up to time, year after year, to the tenth of a second.

How, then, can we save our theory by which we set so much, and rightly, because it has helped us so effectively in studying Nature? If we may be allowed such an extravagant solecism, let us suppose that the engineer of our previous illustration was the hero of a fairy tale. He has built a mill, set a steam-engine in the basement, arranged his spindles above, and is connecting the pulleys by the usual belts, when some stern necessity requires him to transmit all the energy with cobwebs. Of course, a good fairy comes to his aid, and what does she do? Simply makes the cobwebs indefinitely strong. So the physicists, not to be outdone by any fairies, make their ether indefinitely elastic, and their theory lands them just here, with a medium filling all space, thousands of times more elastic than steel, and thousands on thousands of times less dense than hydrogen gas. There

must be a fallacy somewhere, and I strongly suspect it is to be found in our ordinary materialistic notions of causation, which involve the old metaphysical dogma, "*nulla actio in distans*," and which in our day have culminated in the famous apothegm of the German materialist, "Kein Phosphor kein Gedanke."

But it is not my purpose to discuss the doctrines of causation, and I have dwelt on the difficulty, which this subject presents in connection with the undulatory theory, solely because I wished you to appreciate the great interest with which scientific men have looked for some direct manifestation of the mechanical action of light. It is true that the ether waves must have dimensions similar to those of the molecules discussed above, and we must expect, therefore, that they would act primarily on the molecules and not on masses of matter. But still the well-known principles of wave motion have led competent physicists to maintain that a more or less considerable pressure ought to be exerted by the ether waves on the surfaces against which they beat, as a partial resultant of the molecular tremors first imparted. Already, in the last century, attempts were made to discover some evidence of such action, and in various experiments the sun's direct rays were concentrated on films, delicately suspended and carefully protected from all other extraneous influences, but without any apparent effect; and thus

the question remained until about three years ago, when the scientific world were startled by the announcement of Mr. Crookes, of London, that, on suspending a small piece of blackened alder pith in the very perfect vacuum which can now be obtained with the mercury pump, invented by Sprengel, he had seen this light body actually repelled by the sun's rays; and they were still more startled, when, after a few further experiments, he presented us with the instrument he called a radiometer, in which the sun's rays do the no inconsiderable work of turning a small wheel. Let us examine for a moment the construction of this remarkable instrument.

The moving part of the radiometer is a small horizontal wheel, to the ends of whose arms are fastened vertical vanes, usually of mica, and blackened on one side. A glass cap forms the hub, and by the glass-blower's art the wheel is inclosed in a glass bulb, so that the cap rests on the point of a cambric needle; and the wheel is so delicately balanced on this pivot that it turns with the greatest freedom. From the interior of the bulb the air is now exhausted by means of the Sprengel pump, until less than $\frac{1}{0000}$ of the original quantity is left, and the only opening is then hermetically sealed. If, now, the sun's light or even the light from a candle shines on the vanes, the blackened surfaces—which are coated with lampblack—are repelled, and, these being

symmetrically placed around the wheel, the several forces conspire to produce the rapid motion which results. The effect has all the appearance of a direct mechanical action exerted by the light, and for some time was so regarded by Mr. Crookes and other eminent physicists, although in his published papers it should be added that Mr. Crookes carefully abstained from speculating on the subject—aiming, as he has since said, to keep himself unbiased by any theory, while he accumulated the facts upon which a satisfactory explanation might be based.

Singularly, however, the first aspects of the new phenomena proved to be wholly deceptive, and the motion, so far from being an effect of the direct mechanical action of the waves of light, is now believed to be a new and very striking manifestation of molecular motion. To this opinion Mr. Crookes himself has come, and, in a recent article, he writes: "Twelve months' research, however, has thrown much light on these actions, and the explanation afforded by the dynamical theory of gases makes what was a year ago obscure and contradictory now reasonable and intelligible."

As is frequently the case in Nature, the chief effect is here obscured by various subordinate phenomena, and it is not surprising that a great difference of opinion should have arisen in regard to the cause of the motion. This would not be an appropriate place to describe the

numerous investigations occasioned by the controversy, many of which show in a most striking manner how easily experimental evidence may be honestly misinterpreted in support of a preconceived opinion. I will, however, venture to trespass further on your patience, so far as to describe the few experiments by which, very early in the controversy, I satisfied my own mind on the subject.

When, two years ago, I had for the first time an opportunity of experimenting with a radiometer, the opinion was still prevalent that the motion of the wheel was a direct mechanical effect of the waves of light, and, therefore, that the impulses came from the outside of the instrument, the waves passing freely through the glass envelope. At the outset, this opinion did not seem to me to be reasonable, or in harmony with well-known facts; for, knowing how great must be the molecular disturbance caused by the sun's rays, as shown by their heating power, I could not believe that a residual action, such as has been referred to, would first appear in these delicate phenomena observed by Mr. Crookes, and should only be manifested in the vacuum of a mercury pump.

On examining the instrument, my attention was at once arrested by the lampblack coating on the alternate surfaces of the vanes; and, from the remarkable power of lampblack to absorb radiant heat, it was evident at once that, whatever other effects the rays from the sun

or from a flame might cause, they must necessarily determine a constant difference of temperature between the two surfaces of the vanes, and the thought at once occurred that, after all, the motion might be a direct result of this difference of temperature—in other words, that the radiometer might be a small heat engine, whose motions, like those of every other heat engine, depend on the difference of temperature between its parts.

But, if this were true, the effect ought to be proportional solely to the heating power of the rays, and a very easy means of roughly testing this question was at hand. It is well known that an aqueous solution of alum, although transmitting light as freely as the purest water, powerfully absorbs those rays, of any source, which have the chief heating power. Accordingly, I interposed what we call an alum cell in the path of the rays shining on the radiometer, when, although the light on the vanes was as bright as before, the motion was almost completely arrested.

This experiment, however, was not conclusive, as it might still be said that the *heat*-giving rays acted *mechanically*, and it must be admitted that the chief part of the energy in the rays, even from the most brilliant luminous sources, always takes the form of heat. But, if the action is mechanical, the reaction must be against the medium through which the rays are transmitted,

while, if the radiometer is simply a heat engine, the action and reaction must be, ultimately at least, between the heater and the cooler, which in this case are respectively the blackened surfaces of the vanes and the glass walls of the inclosing bulb; and here, again, a very easy method of testing the actual condition at once suggested itself.

If the motion of the radiometer wheel is an effect of mechanical impulses transmitted in the direction of the beam of light, it was certainly to be expected that the beam would act on the lustrous as well as on the blackened mica surfaces, however large might be the difference in the resultants producing mechanical motion, in consequence of the great absorbing power of the lampblack. Moreover, since the instrument is so constructed that, of two vanes on opposite sides of the wheel, one always presents a blackened and the other a lustrous surface to an incident beam, we should further expect to find in the motion of the wheel a differential phenomenon, due to the unequal action of the light on these surfaces. On the other hand, if the radiometer is a heat engine, and the reaction takes place between the heated blackened surfaces of the vanes and the colder glass, it is evident that the total effect will be simply the sum of the effects at the several surfaces.

In order to investigate the question thus presented, I placed the radiometer before a common kerosene lamp,

and observed, with a stop-watch, the number of seconds that elapsed during ten revolutions of the little wheel. Finding that this number was absolutely constant, I next screened one half of the bulb, so that only the blackened faces were exposed to the light as the wheel turned them into the beam. Again, I several times observed the number of seconds during ten turns, which, although equally constant, was greater than before. Lastly, I screened the blackened surfaces so that, as the wheel turned, only the lustrous surfaces of mica were exposed to the light, when, to my surprise, the wheel continued to turn in the same direction as before, although much more slowly. It appeared as if the lustrous surfaces were attracted by the light. Again I observed the time of ten revolutions, and here I have collected my results, reducing them, in the last column, so as to show the corresponding number of revolutions in the same time:

CONDITIONS.	Time of ten revolutions.	No. of revolutions in same time.
Both faccs exposed.............	8 seconds.	319
Blackened faces only...........	11 "	232
Mica faces only................	29 "	88

It will be noticed that 88 + 232 equals very nearly 319. Evidently the effect, so far from being differential, is concurrent. Hence, the action which causes the motion must take place between the parts of the instru-

ment, and can not be a direct effect of impulses imparted by ether waves; or else we are driven to the most improbable alternative, that lampblack and mica should have such a remarkable selective power that the impulses imparted by the light should exert a repulsive force at one surface and an attractive force at the other. Were there, however, such an improbable effect, it must be independent of the thickness of the mica vanes; while, on the other hand, if, as seemed to us now most probable, the whole effect depended on the difference of temperature between the lampblack and the mica, and if the light produced an effect on the mica surface only because, the mica plate being diathermous to a very considerable extent, the lampblack became heated through the plate more than the plate itself, then it would follow that, if we used a thicker mica plate, which would absorb more of the heat, we ought to obtain a marked difference of effect. Accordingly, we repeated the experiment with an equally sensitive radiometer, which we made for the purpose, with comparatively thick vanes, and with this the effect of a beam of light on the mica surface was absolutely null, the wheel revolving in the same time, whether these faces were protected or not.

But one thing was now wanting to make the demonstration complete. A heat engine is reversible, and if the motion of the radiometer depended on the circum-

stance that the temperature of the blackened faces of the vanes was higher than that of the glass, then by reversing the conditions we ought to reverse the motion. Accordingly, I carefully heated the glass bulb over a lamp, until it was as hot as the hand would bear, and then placed the instrument in a cold room, trusting to the great radiating power of lampblack to maintain the temperature of the blackened surfaces of the vanes below that of the glass. Immediately the wheel began to turn in the opposite direction, and continued to turn until the temperature of the glass came into equilibrium with the surrounding objects.

These early experiments have since been confirmed to the fullest extent, and no physicist at the present day can reasonably doubt that the radiometer is a very beautiful example of a heat engine, and it is the first that has been made to work continuously by the heat of the sunbeam. But it is one thing to show that the instrument is a heat engine, and quite another thing to explain in detail the manner in which it acts. In regard to the last point, there is still room for much difference of opinion, although physicists are generally agreed in referring the action to the residual gas that is left in the bulb. As for myself, I became strongly persuaded—after experimenting with more than one hundred of these instruments, made under my own eye, with every variation of

condition I could suggest—*that the effect was due to the same cause which determines gas pressure*, and, according to the dynamical theory of gases, this amounts to saying that the effect is due to molecular motion. I have not time, however, to describe either my own experiments on which this opinion was first based, or the far more thorough investigations since made by others, which have served to strengthen the first impression.* But, after our previous discussions, a few words will suffice to show how the molecular theory explains the new phenomena.

Although the air in the bulb has been so nearly exhausted that less than the one-thousandth part remains, yet it must be borne in mind that the number of molecules left behind is by no means inconsiderable. As will be seen by referring to our table, there must still be no less than 311,000 million million in every cubic inch. Moreover, the absolute pressure which this residual gas exerts is a very appreciable quantity. It is simply the one-thousandth of the normal pressure of the atmosphere, that is, of $14\frac{7}{10}$ pounds on a square inch, which is equivalent to a little over one hundred grains on the same area. Now, the area of the blackened surfaces of the vanes of an ordinary radiometer measures just about a square inch, and the wheel is mounted so delicately that

* See notice of these investigations by the author of this article, in "American Journal of Science and Arts," September, 1877 (3), xiv, 231.

a constant pressure of one-tenth of a grain would be sufficient to produce rapid motion. So that a difference of pressure on the opposite faces of the vanes, equal to one one-thousandth of the whole amount, is all that we need account for; and, as can easily be calculated, a difference of temperature of less than half a degree Fahrenheit would cause all this difference in the pressure of the rarefied air.

But you may ask, How can such a difference of pressure exist on different surfaces exposed to one and the same medium? and your question is a perfectly legitimate one; for it is just here that the new phenomena seem to belie all our previous experience. If, however, you followed me in my very partial exposition of the mechanical theory of gases, you will easily see that on this theory it is a more difficult question to explain why such a difference of pressure does not manifest itself in every gas medium and under all conditions between any two surfaces having different temperatures.

We saw that gas pressure is a double effect, caused both by the impact of molecules and by the recoil of the surface attending their rebound. We also saw that when molecules strike a heated surface they rebound with increased velocity, and hence produce an increased pressure against the surface, the greater the higher the temperature. According to this theory, then, we should expect

to find the same atmosphere pressing unequally on equal surfaces if at different temperatures; and the difference in the pressure on the lampblack and mica surfaces of the vanes, which the motion of the radiometer wheel necessarily implies, is therefore simply the normal effect of the mechanical condition of every gas medium. The real difficulty is, to explain why we must exhaust the air so perfectly before the effect manifests itself.

The new theory is equal to the emergency. As has been already pointed out, in the ordinary state of the air the amplitude of the molecular motion is exceedingly small, not over a few ten-millionths of an inch—a very small fraction, therefore, of the height of the inequalities on the lampblack surfaces of the vanes of a radiometer. Under such circumstances, evidently the molecules would not leave the heated surface, but simply bound back and forth between the vanes and the surrounding mass of dense air, which, being almost absolutely a non-conductor of heat, must act essentially like an elastic solid wall confining the vanes on either side. For the time being, and until replaced by convection currents, the oscillating molecules are as much a part of the vanes as our atmosphere is a part of the earth; and on this system, as a whole, the homogeneous dense air which surrounds it must press equally from all directions. In proportion, however, as the air is exhausted, the molecules find more

room and the amplitude of the molecular motion is increased, and, when a very high degree of exhaustion is reached, the air particles no longer bound back and forth on the vanes without change of condition, but they either bound off entirely like a ball from a cannon, or else, having transferred a portion of their momentum, return with diminished velocity, and in either case the force of the reaction is felt.*

* The reader will, of course, distinguish between the differential action on the opposite faces of the vanes of the radiometer and the reaction between the vanes and the glass which are the heater and the cooler of the little engine. Nor will it be necessary to remind any student that a popular view of such a complex subject must be necessarily partial. In the present case we not only meet with the usual difficulties in this respect, but, moreover, the principles of molecular mechanics have not been so fully developed as to preclude important differences of opinion between equally competent authorities in regard to the details of the theory. To avoid misapprehension, we may here add that, in order to obtain in the radiometer a reaction between the heater and the cooler, it is not necessary that the space between them should actually be crossed by the moving molecules. It is only necessary that the momentum should be transferred across the space, and this may take place along lines consisting of many molecules each. The theory, however, shows that such a transfer can only take place in a highly rarefied medium. In an atmosphere of ordinary density, the accession of heat which the vanes of a radiometer might receive from a radiant source would be diffused through the mass of the inclosed air. This amounts to saying that the momentum would be so diffused, and hence, under such circumstances, the molecular motion would not determine any reaction between the vanes and the glass envelope. Indeed, a dense mass of gas presents to the conduction of heat, which represents momentum, a wall far more impenetrable than the surrounding glass, and the diffusion of heat is almost wholly brought about by convection currents which rise from the heated surfaces. It will thus be seen that the great non-con-

Thus it appears that we have been able to show by very definite experimental evidence that the radiometer is a heat engine. We have also been able to show that such a difference of temperature as the radiation must produce in the air in *direct* contact with the opposite faces of the vanes of the radiometer would determine a difference of tension, which is sufficient to account for the motion of the wheel. Finally, we have shown, as fully as is possible in a popular lecture, that, according to the mechanical theory of gases, such a difference of tension would have its normal effect only in a highly rarefied atmosphere, and thus we have brought the new phenomena into harmony with the general principles of molecular mechanics previously established.

More than this can not be said of the steam-engine, although, of course, in the older engine the measure-

ducting power of air comes into play to prevent not only the transfer of momentum from the vanes to the glass, but also, almost entirely, any direct transfer to the surrounding mass of gas. Hence, as stated above, the heated molecules bound back and forth on the vanes without change of condition, and the mass of the air retains its uniform tension in all parts of the bulb, except in so far as this is slowly altered by the convection currents just referred to. As the atmosphere, however, becomes less dense, the diffusion of heat by convection diminishes, and that by molecular motion (conduction) increases until the last greatly predominates. When, now, the exhaustion reaches so great a degree that the heat, or momentum, is rapidly transferred from the heater to the cooler by an exaggeration, or, possibly, a modification, of the mode of action we call conduction, then we have the reaction on which the motion of the radiometer wheel depends.

ments on which the theory is based are vastly more accurate and complete. But the moment we attempt to go beyond the general principles of heat engines, of which the steam-engine is such a conspicuous illustration, and explain how the heat is transformed into motion, we have to resort to the molecular theory just as in the case of the radiometer; and the motion of the steam-engine seems to us less wonderful than that of the radiometer only because it is more familiar and more completely harmonized with the rest of our knowledge. Moreover, the very molecular theory which we call upon to explain the steam-engine involves consequences which, as we have seen, have been first realized in the radiometer; and thus it is that this new instrument, although disappointing the first expectations of its discoverer, has furnished a very striking confirmation of this wonderful theory. Indeed, the confirmation is so remote and yet so close, so unexpected and yet so strong, that the new phenomena almost seem to be a direct manifestation of the molecular motion which our theory assumes; and when a new discovery thus confirms the accuracy of a previous generalization, and gives us additional reason to believe that the glimpses we have gained into the order of Nature are trustworthy, it excites, with reason, among scientific scholars the warmest interest.

And when we consider the vast scope of the molecu-

lar theory, the order on order of existences which it opens to the imagination, how can we fail to be impressed with the position in which it places man midway between the molecular cosmos on the one side and the stellar cosmos on the other—a position in which he is able, in some measure at least, to study and interpret both?

Since the time to which we referred at the beginning of this lecture, when man's dwelling-place was looked at as the center of a creation which was solely subservient to his wants, there has been a reaction to the opposite extreme, and we have heard much of the utter insignificance of the earth in a universe among whose immensities all human belongings are but as a drop in the ocean. When now, however, we learn from Sir William Thomson that the drop of water in our comparison is itself a universe, consisting of units so small that, were the drop magnified to the size of the earth, these units would not exceed in magnitude a cricket-ball,* and when, on studying chemistry, we still further learn that these units are not single masses but systems of atoms, we may leave the illusions of the imagination from the one side to correct those from the other, and all will teach us the great lesson that man's place in Nature is not to be estimated by relations of magnitude, but

* "Nature," No. 22, March 31, 1870.

by the intelligence which makes the whole creation his own.

But, if it is man's privilege to follow both the atoms and the stars in their courses, he finds that, while thus exercising the highest attributes of his nature, he is ever in the presence of an immeasurably superior intelligence, before which he must bow and adore, and thus come to him both the assurance and the pledge of a kinship in which his only real glory can be found.

V.

MEMOIR OF THOMAS GRAHAM.

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It would be difficult to find in the history of science a character more simple, more noble, or more symmetrical in all its parts than that of Thomas Graham, and he will always be remembered as one of the most eminent of those great students of nature who have rendered our Saxon race illustrious. He was born of Scotch parents in Glasgow in the year 1805, and in that city, where he received his education, all his early life was passed. In 1837 he went to London as Professor of Chemistry in the newly established London University, now called University College, and he occupied this chair until the year 1855, when he succeeded Sir John Herschel as Master of the Royal Mint, a post which he held to the close of his life. His death, on the 16th of September

last (1869), at the age of sixty, was caused by no active disease, but was simply the wearing out of a constitution enfeebled in youth by privations voluntarily and courageously encountered that he might devote his life to scientific study. As with all earnest students, that life was uneventful, if judged by ordinary standards; and the records of his discoveries form the only materials for his biography.

Although one of the most successful investigators of physical science, the late Master of the Mint had not that felicity of language or that copiousness of illustration which added so much to the popular reputation of his distinguished contemporary, Faraday; but his influence on the progress of science was not less marked or less important. Both of these eminent men were for a long period of years best known to the English public as teachers of chemistry, but their investigations were chiefly limited to physical problems; yet, although both cultivated the border ground between chemistry and physics, they followed wholly different lines of research. While Faraday was so successfully developing the principles of electrical action, Graham with equal success was investigating the laws of molecular motion. Each followed with wonderful constancy, as well as skill, a single line of study from first to last, and to this concentration of power their great discoveries are largely due.

One of the earliest and most important of Graham's investigations, and the one which gave the direction to his subsequent course of study, was that on the diffusion of gases. It had already been recognized that impenetrability in its ordinary sense is not, as was formerly supposed, a universal quality of matter. Dalton had not only recognized that aëriform bodies exhibit a positive tendency to mix, or to penetrate through each other, even in opposition to the force of gravity, but had made this quality of gases the subject of experimental investigation. He inferred, as the result of his inquiry, "that different gases afford no resistance to each other; but that one gas spreads or expands into the space occupied by another gas, as it would rush into a vacuum; at least, that the resistance which the particles of one gas offer to those of another is of a very imperfect kind, to be compared to the resistance which stones in the channel of a stream oppose to the flow of running water." But, although this theory of Dalton was essentially correct and involved the whole truth, yet it was supported by no sufficient evidence, and he failed to perceive the simple law which underlies this whole class of phenomena.

Graham, "on entering on this inquiry, found that gases diffuse into the atmosphere with different degrees of ease and rapidity." This was first observed by allowing each gas to diffuse from a bottle into the air through

a narrow tube in opposition to the solicitation of gravity. Afterward an observation of Doebereiner on the escape of hydrogen gas by a fissure or crack in a glass receiver caused him to vary the conditions of his experiments, and led to the invention of the well-known "diffusion tube." In this simple apparatus a thin septum of plaster of Paris is used to separate the diffusing gases, which, while it arrests in a great measure all direct currents between the two media, does not interfere with the molecular motion. Much later, Graham found in prepared graphite a material far better adapted to this purpose than the plaster, and he used septa of this mineral to confirm his early results, in answer to certain ill-considered criticisms in Bunsen's work on gasometry. These septa he was in the habit of calling his "atomic filters."

By means of the diffusion tube, Graham was able to measure accurately the relative times of diffusion of different gases, and he found that *equal volumes of any two gases interpenetrate each other in times which are inversely proportional to the square roots of their respective densities;* and this fundamental law was the greatest discovery of our late foreign associate. It is now universally recognized as one of the few great cardinal principles which form the basis of physical science.

It can be shown, on the principles of pneumatics,

that gases should rush into a vacuum with velocities corresponding to the numbers which have been found to express their diffusion times; and, in a series of experiments on what he calls the "*effusion*" of gases, Graham confirmed by trial this deduction of theory. In these experiments a measured volume of the gas was allowed to find its way into the vacuous jar through a minute aperture in a thin metallic plate, and he carefully distinguished between this class of phenomena and the flowing of gases through capillary tubes into a vacuum, in which case, however short the tube, the effects of friction materially modify the result. This last class of phenomena Graham likewise investigated, and designated by the term "transpiration."

While, however, it thus appears that the results of Graham's investigation were in strict accordance with Dalton's theory, it must also be evident that Graham was the first to observe the exact numerical relation which obtains in this class of phenomena, and that all-important circumstance entitles him to be regarded as the discoverer of the law of diffusion. The law, however, at first enunciated, was purely empirical, and Graham himself says that something more must be assumed than that gases are vacua to each other, in order to explain all the phenomena observed; and according to his original view this representation of the process was only a convenient

mode of expressing the final result. Such has proved to be the case.

Like other great men, Graham built better than he knew. In the progress of physical science during the last twenty-five years, two principles have become more and more conspicuous, until at last they have completely revolutionized the philosophy of chemistry. In the first place, it has appeared that a host of chemical as well as of physical facts are coördinated by the assumption that all substances in the state of gas have the same molecular volume, or, in other words, contain the same number of molecules in a given space; and in the second place, it has become evident that the phenomena of heat are simply the manifestations of molecular motion. According to this view, the temperature of a body is the *vis viva* of its molecules; and, since all molecules at a given temperature have the same *vis viva*, it follows that the molecules must move with velocities which are inversely proportional to the square roots of the molecular weights. Moreover, since the molecular volumes are equal, and the molecular weights therefore proportional to the densities of the aëriform bodies in which the molecules are the active units, it also follows that the velocities of the molecules in any two gases are inversely proportional to the square roots of their respective densities. Thus the simple numerical relations first observed in the phe-

nomena of diffusion are the direct result of molecular motion; and it is now seen that Graham's empirical law is included under the fundamental laws of motion. Thus Graham's investigation has become the basis of the new science of molecular mechanics, and his measurements of the rates of diffusion prove to be the measures of molecular velocities.

From the study of diffusion Graham passed by a natural transition to the investigation of a class of phenomena which, although closely allied to the first as to the effects produced, differ wholly in their essential nature. Here also he followed in the footsteps of Dalton. This distinguished chemist had noticed that a bubble of air separated by a film of water from an atmosphere of carbonic anhydride gradually expanded until it burst. In like manner a moist bladder, half filled with air and tied, if suspended in an atmosphere of the same material, becomes in time greatly distended by the insinuation of this gas through its substance. This effect can not be the result of simple diffusion, for it is to be remembered that the thinnest film of water, or of any liquid, is absolutely impermeable to a gas as such, and, moreover, only the carbonic anhydride passes through the film, very little or none of the air escaping outward. The result depends, first, upon the solution of the carbonic anhydride by the water on one surface of the film; secondly, on

the evaporation into the air, from the other surface, of the gas thus absorbed. Similar experiments were made by Drs. Mitchell and Faust, and others, in which gases passed through a film of India-rubber, entering into a partial combination with the material on one surface, and escaping from it on the other.

Graham not only considerably extended our knowledge of this class of phenomena, but also gave us a satisfactory explanation of the mode in which these remarkable results are produced. He recognized in these cases the action of a feeble chemical force, insufficient to produce a definite compound, but still capable of determining a more or less perfect union, as in the case of simple solution. He also distinguished the influence of mass in causing the formation or decomposition of such weak chemical compounds. The conditions of the phenomena under consideration are simply these:

First. A material for the septum capable of forming a feeble chemical union with the gas to be transferred.

Secondly. An excess of the gas on one side of the film and a deficiency on the other.

Thirdly. Such a temperature that the unstable compound may form at the surface, where the aëriform constituent is present in large mass, while it decomposes at the opposite surface, where the quantity is less abundant.

One of the most remarkable results of Graham's study

of this peculiar mode of transfer of aëriform matter through the very substance of solid bodies was an ingenious method of separating the oxygen from the atmosphere. The apparatus consisted simply of a bag of India-rubber kept distended by an interior framework, while it was exhausted by a Sprengel pump. Under these circumstances the selective affinity of the caoutchouc determines such a difference in the rate of transfer of the two constituents of the atmosphere that the amount of oxygen in the transpired air rises to forty per cent., and by repeating the process nearly pure oxygen may be obtained. It was at first hoped that this method might find a valuable application in the arts, but in this Graham was disappointed; for the same result has since been effected by purely chemical methods, which are both cheaper and more rapid.

These experiments on India-rubber naturally led to the study of similar effects produced with metallic septa, which, although to some extent previously observed in passing gases through heated metallic tubes, had been only imperfectly understood. Thus, when a stream of hydrogen or carbonic oxide is passed through a red-hot iron tube, a no inconsiderable portion of the gas escapes through the walls. The same is true to a still greater degree when hydrogen is passed through a red-hot tube of platinum, and Graham showed that, through the walls of

a tube of palladium, hydrogen gas passes, under the same conditions, almost as rapidly as water through a sieve. Moreover, our distinguished associate proved that this rapid transfer of gas through these dense metallic septa was due, as in the case of the India-rubber, to an actual chemical combination of its material with the metal, formed at the surface, where the gas is in excess, and as rapidly decomposed on the opposite face of the septum. He not only recognized as belonging to this class of phenomena the very great absorption of hydrogen by platinum plate and sponge in the familiar experiment of the Doebereiner lamp, but also showed that this gas is a definite constituent of meteoric iron—a fact of great interest from its bearing on the meteoric theory.

We are thus led to Graham's last important discovery, which was the justification of the theory we have been considering, and the crowning of this long line of investigation. As may be anticipated from what has been said, the most marked example of that order of chemical compounds, to which the metallic transpiration of aëriform matter we have been considering is due, is the compound of palladium with hydrogen. Graham showed that, when a plate of this metal is made the negative pole in the electrolysis of water, it absorbs nearly one thousand times its volume of hydrogen gas—a quantity approximatively equivalent to one atom of hydrogen

to each atom of palladium. He further showed that the metal thus becomes so profoundly altered as to indicate that the product of this union is a definite compound. Not only is the volume of the metal increased, but its tenacity and conducting power for electricity are diminished, and it acquires a slight susceptibility to magnetism, which the pure metal does not possess. The chemical qualities of this product are also remarkable. It precipitates mercury from a solution of its chloride, and in general acts as a strong reducing agent. Exposed to the action of chlorine, bromine, or iodine, the hydrogen leaves the palladium and enters into direct union with these elements. Moreover, although the compound is readily decomposed by heat, the gas can not be expelled from the metal by simple mechanical means.

These facts recall the similar relations frequently observed between the qualities of an alloy and those of the constituent metals, and suggest the inference made by Graham, that palladium charged with hydrogen is a compound of the same class—a conclusion which harmonizes with the theory long held by many chemists, that hydrogen gas is the vapor of a very volatile metal. This element, however, when combined with palladium, is in a peculiarly active state, which sustains somewhat the same relation to the familiar gas that ozone bears to ordinary oxygen. Hence Graham distinguished this condition of

hydrogen by the term "hydrogenium." Shortly before his death a medal was struck at the Royal Mint from the hydrogen palladium alloy in honor of its discovery; but, although this discovery attracted public attention chiefly on account of the singular chemical relations of hydrogen, which it brought so prominently to notice, it will be remembered in the history of science rather as the beautiful termination of a life-long investigation, of which the medal was the appropriate seal.

Simultaneously with the experiments on *gases*, whose results we have endeavored to present in the preceding pages, Graham carried forward a parallel line of investigation of an allied class of phenomena, which may be regarded as the manifestations of molecular motion in *liquid* bodies. The phenomena of diffusion reappear in liquids, and Graham carefully observed the times in which equal weights of various salts dissolved in water diffused from an open-mouth bottle into a large volume of pure water, in which the bottle was immersed. He was not, however, able to correlate the results of these experiments by such a simple law as that which obtains with gases. It appeared, nevertheless, that the rate of diffusion differs very greatly for the different soluble salts, having some relation to the chemical composition of the salt which he was unable to discover. But he found it possible to divide the salts into groups of equi-diffusive substances, and

he showed that the rate of diffusion of the several groups bear to one another simple numerical ratios.

More important results were obtained from the study of a class of phenomena corresponding to the transpiration of gases through India-rubber or metallic septa. These phenomena, as manifested in the transfer of liquids and of salts in solution through bladder or a similar membrane, had previously been frequently studied under the names of exosmose and endosmose, but to Graham we owe the first satisfactory explanation. As in the case of gases, he referred these effects to the influence of chemical force, combination taking place on one surface of the membrane and the compound breaking up on the other, the difference depending, as in the previous instance, on the influence of mass. He also swept away the arbitrary distinctions made by previous experimenters, showed that this whole class of phenomena are essentially similar, and called this manifestation of power simply "osmose."

While studying osmotic action, Graham was led to one of his most important generalizations—the recognition of the crystalline and amorphous states as fundamental distinctions in chemistry. Bodies in the first state he called crystalloids; those in the last state, colloids (resembling glue). That there is a difference in strucure between crystalloids, like sugar or felspar, and col-

loids, like barley candy or glass, has of course always been evident to the most superficial observer; but Graham was the first to recognize in these external differences two fundamentally distinct conditions of matter not peculiar to certain substances, but underlying all chemical differences, and appearing to a greater or less degree in every substance. He showed that the power of diffusion through liquids depends very much on these fundamental differences of condition—sugar, one of the least diffusible of the crystalloids, diffusing fourteen times more rapidly than caromel, the corresponding colloid. He also showed that, in accordance with the general chemical rule, while colloids readily combine with crystalloids, bodies in the same condition manifest little or no tendency to chemical union. Hence, in osmose, where the membranes employed are invariably colloidal, the osmotic action is confined almost entirely to crystalloids, since they alone are capable of entering into that combination with the material of the septum on which the whole action depends.

On the above principles Graham based a simple method of separating crystalloids from colloids, which he calls "dialysis," and which was a most valuable addition to the means of chemical analysis. A shallow tray, prepared by stretching parchment paper (an insoluble colloid) over a gutta-percha hoop, is the only apparatus re-

quired. The solution to be "dialyzed" is poured into this tray, which is then floated on pure water, whose volume should be eight or ten times greater than that of the solution. Under these conditions the crystalloids will diffuse through the porus septum into the water, leaving the colloids on the tray, and in the course of a few days a more or less complete separation of the two classes of bodies will have taken place. In this way arsenious acid and similar crystalloids may be separated from the colloidal materials with which, in the case of poisoning, they are usually found mixed in the animal juices or tissues.

But, besides having these practical applications, the method of dialysis in the hands of Graham yielded the most startling results, developing an almost entirely new class of bodies, as the colloidal forms of our most familiar substances, and justifying the conclusion that the colloidal as well as the crystalline condition is an almost universal attribute of matter. Thus, he was able to obtain solutions in water of the colloidal states of aluminic, fevric, chromic, stannic, metastannic, titanic, molybdic, tungstic, and silicic hydrates, all of which gelatinize under definite conditions like a solution of glue. The wonderful nature of these facts can be thoroughly appreciated only by those familiar with the subject, but all may understand the surprise with which the chemist saw such hard, in-

soluble bodies as flint dissolved abundantly in water and converted into soft jellies. These facts are, without doubt, the most important contributions of Dr. Graham to pure chemistry.

In this sketch of the scientific career of our late associate, we have followed the logical, rather than the chronological, order of events, hoping thus to render the relations of the different parts of his work more intelligible. It must be remembered, however, that the two lines of investigation we have distinguished were in fact interwoven, and that the beautiful harmony which his completed life presents was the result, not of a preconceived plan, but of a constant devotion to truth, and a childlike faith, which unhesitatingly pressed forward whenever nature pointed out the way.

Although the investigations of the phenomena connected with the molecular motion in gases and liquids were by far the most important of Dr. Graham's labors, he also contributed to chemistry many researches which can not be included under this head. Of these, which we may regard as his detached efforts, the most important was his investigation of the hydrates and other salts of phosphorus. It is true that the interpretation he gave of the results has been materially modified by the modern chemical philosophy, yet the facts which he established form an important part of the basis on which

that philosophy rests. Indeed, it seems as if he almost anticipated the later doctrines of types and polybasic acids, and in none of his work did he show more discriminating observation or acute reasoning. A subsequent investigation on the condition of water in several crystalline salts and in the hydrates of sulphuric acid is equally remarkable. Lastly, Graham also made interesting observations on the combination of alcohol with salts, on the process of etherification, on the slow oxidation of phosphorus, and on the spontaneous inflammability of phosphureted hydrogen. It would not, however, be appropriate in this place to do more than enumerate the subjects of these less important studies; and we have therefore only aimed in this sketch to give a general view of the character of the field which this eminent student of nature chiefly cultivated, and to show how abundant was the harvest of truth which we owe to his faithful toil.

Graham was not a voluminous writer. His scientific papers were all very brief, but comprehensive, and his "Elements of Chemistry" was his only large work. This was an admirable exposition of chemical physics, as well as of pure chemistry, and gave a more philosophical account of the theory of the galvanic battery than had previously appeared. Our late associate was fortunate in receiving during life a generous recognition of

the value of his labors. His membership was sought by almost all the chief scientific societies of the world, and he enjoyed to a high degree the confidence and esteem of his associates. Indeed, he was singularly elevated above the petty jealousies and belittling quarrels which so often mar the beauty of a student's life, while the great loveliness and kindliness of his nature closely endeared him to his friends.

In concluding, we must not forget to mention that most genial trait of Graham's character, his sympathy with young men, which gave him great influence as a teacher in the college with which he was long associated. There are many now prominent in the scientific world who have found in his encouragement the strongest incentive to perseverance, and in his approval and friendship the best reward of success.

VI.

MEMOIR OF WILLIAM HALLOWES MILLER.

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William Hallowes Miller, who was elected Foreign Honorary Member of this Academy in the place of C. F. Naumann, May 26, 1874, died at his residence in Cambridge, England, on the 20th of May, 1880, at the age of seventy-nine, having been born at Velindre, in Wales, April 5, 1801. His life was singularly uneventful, even for a scholar. Graduating with mathematical honors at Cambridge in 1826, he became a fellow of his college (St. John's) in 1829, and was elected Professor of Mineralogy in the University in 1832. Under the influence of the calm and elegant associations of this ancient English university, Miller passed a long and tranquil life —crowded with useful labors, honored by the respect and

love of his associates, and blessed by congenial family ties. This quiet student-life was exactly suited to his nature, which shunned the bustle and unrest of our modern world. For relaxation, even, he loved to seek the retired valleys of the Eastern Alps; and the description which he once gave to the writer, of himself sitting at the side of his wife amid the grand scenery, intent on developing crystallographic formulæ, while the accomplished artist traced the magnificent outlines of the Dolo mite mountains, was a beautiful idyl of science.

Miller's activities, however, were not confined to the University. In 1838 he became a Fellow of the Royal Society, and in 1856 he was appointed its Foreign Secretary—a post for which he was eminently fitted, and which he filled for many years. In 1843 he was selected one of a committee to superintend the construction of the new Parliamentary standards of length and weight, to replace those which had been lost in the fire which consumed the Houses of Parliament in 1834, and to Professor Miller was confided the construction of the new standard of weight. His work on this important committee, described in an extended paper published in the "Philosophical Transactions" for 1856, was a model of conscientious investigation and scientific accuracy. Professor Miller was subsequently a member of a new Royal Commission for "examining into and reporting on the

state of the secondary standards, and for considering every question which could affect the primary, secondary, and local standards"; and in 1870 he was appointed a member of the "Commission Internationale du Mètre." His services on this commission were of great value, and it has been said that "there was no member whose opinions had greater weight in influencing a decision upon any intricate and delicate question."

Valuable, however, as were Professor Miller's public services on these various commissions, his chief work was at the University. His teacher, Dr. William Whewell—afterward the Master of Trinity College—was his immediate predecessor in the Professorship of Mineralogy at Cambridge. This great scholar, whose encyclopædic mind could not long be confined in so narrow a field, held the professorship only four years; but during this period he devoted himself with his usual enthusiasm to the study of crystallography, and he accomplished a most important work in attracting to the same study young Miller, who brought his mathematical training to its elucidation. It was the privilege of Professor Miller to accomplish a unique work, for the like of which a more advanced science, with its multiplicity of details, will offer few opportunities.

The foundations of crystallography had been laid long before Miller's time. Haüy is usually regarded as the

founder of the science; for he first discovered the importance of cleavage, and classed the known facts under a definite system. Taking cleavage as his guide, and assuming that the forms of cleavage were not only the *primitive forms* of crystals as a whole, but also the forms of their *integrant molecules*, he endeavored to show that all secondary forms might be derived from a few primary forms, regarded as elements of nature, by means of *decrements* of molecules at their edges. In like manner he showed that all the forms of a given mineral, like fluor-spar or calcite, might be built up from the integrant molecules by skillfully placing together the primitive forms. Haüy's dissection of crystals, in a manner which appeared to lead to their ultimate crystalline elements, gained for his system great popular attention and applause. The system was developed with great perspicuity and completeness in a work remarkable for the vivacity of its style and the felicity of its illustration. Moreover, a simple mathematical expression was given to the system, and the notation which Haüy invented to express the relation of the secondary to the primary forms, as modified and improved by Lèvy, is still used by the French mineralogists.

The system of Haüy, however, was highly artificial, and only prepared the way for a simpler and more general expression of the facts. The German crystallogra-

pher, Weiss, seems to be the first to have recognized the truth that the decrements of Haüy were merely a mechanical mode of representing the fact that all the secondary faces of a crystal make intercepts on the edges of the primitive form which are simple multiples of each other; and, this general conception once gained, it was soon seen that these ratios could be as simply measured on the axes of symmetry of the crystal as on the edges of the fundamental forms; and, moreover, that, when crystal forms are viewed in their relation to these axes, a more general law becomes evident, and the artificial distinction between primary and secondary forms disappears.

Thus became slowly evolved the conception of a crystal as a group of similar planes symmetrically disposed around certain definite and obvious systems of axes, and so placed that the intercepts, or parameters, on these axes bore to each other a simple numerical ratio. Representing by $a:b:c$ the ratio of the intercepts of a plane on the three axes of a crystal of a given substance, then the intercepts of every other plane of this, or of any other crystal of the same substance, conform to the general proportion $m\,a:n\,b:p\,c$, in which m, n, p are three simple whole numbers. This simple notation, devised by Weiss, expressed the fundamental law of crystallography; and the conception of a crystal as a system of planes, symmetrically distributed according to this law,

was a great advance beyond the decrements of Haüy, an advance not unlike that of astronomy from the system of vortices to the law of gravitation. Yet, as the mechanism of vortices was a natural prelude to the law of Newton, so the decrements of Haüy prepared the way for the wider views of the German crystallographers.

Whether Weiss or Mohs contributed most to advance crystallography to its more philosophical stage, it is not important here to inquire. Each of these eminent scholars did an important work in developing and diffusing the larger ideas, and in showing by their investigations that the facts of nature corresponded to the new conceptions. But to Carl Friedrich Naumann, Professor at the time in the "Bergakademie zu Freiberg," belongs the merit of first developing a complete system of theoretical crystallography based on the laws of symmetry and axial ratios. His "Lehrbuch der reinen und angewandten Krystallographie," published in two volumes at Leipzig in 1830, was a remarkable production, and seemed to grasp the whole theory of the external forms of crystals. Naumann used the obvious and direct methods of analytical geometry to express the quantitative relations between the parts of a crystal; and, although his methods are often unnecessarily prolix and his notation awkward, his formulæ are well adapted to calculation, and easily intelligible to persons moderately disciplined in mathematics.

But, however comprehensive and perfect in its details, the system of Naumann was cumbrous, and lacked elegance of mathematical form. This arose chiefly from the fact that the old methods of analytical geometry were unsuited to the problems of crystallography; but it resulted also from a habit of the German mind to dwell on details and give importance to systems of classification. To Naumann the six crystalline systems were as much realities of nature as were the forms of the integrant molecules to Haüy, and he failed to grasp the larger thought which includes all partial systems in one comprehensive plan.

Our late colleague, Professor Miller, on the other hand, had that power of mathematical generalization which enabled him to properly subordinate the parts to the whole, and to develop a system of mathematical crystallography of such simplicity and beauty of form that it leaves little to be desired. This was the great work of his life, and a work worthy of the university which had produced the "Principia." It was published in 1839, under the title, "A Treatise on Crystallography"; and in 1863 the substance of the work was reproduced in a more perfect form, still more condensed and generalized, in a thin volume of only eighty-six pages, which the author modestly called, "A Tract on Crystallography."

Miller began his study of crystallography with the

same materials as Naumann; but, in addition, he adopted the beautiful method of Franz Ernst Neumann of referring the faces of a crystal to the surface of a circumscribed sphere by means of radii drawn perpendicular to the faces. The points where the radii meet the spherical surface are the poles of the faces, and the arcs of great circles connecting these poles may obviously be used as a measure of the angles between the crystal faces. This invention of Neumann's was the germ of Miller's system of crystallography, for it enabled the English mathematician to apply the elegant and compendious methods of spherical trigonometry to the solution of crystallographic problems; and Professor Miller always expressed his great indebtedness to Neumann, not only for this simple mode of defining the position of the faces of a crystal, but also for his method of representing the relative position of the poles of the faces on a plane surface by a beautiful application of the methods of stereographic and gnomonic projection. This method of representing a crystal shows very clearly the relations of the parts, and was undoubtedly of great aid to Miller in assisting him to generalize his deductions.

From the outset, Professor Miller apprehended more clearly than any previous writer the all-embracing scope of the great law of crystallography. He opens his treatise with its enunciation, and, from this law as the funda-

mental principle of the subject, the whole of his system of crystallography is logically developed. Beyond this, all that is peculiar to Miller's system is involved in two or three general theorems. The rest of his treatise consists of deductions from these principles and their application to particular cases.

One of the most important of these principles, and one which in the treatise is involved in the enunciation of the fundamental law of crystallography, is in its essence nothing but an analytical device. As we have already stated, Weiss had shown that, if $a:b:c$ represent the ratio of the intercepts of any plane of a crystal on the three axes x, y, and z, respectively, the intercepts of any other possible plane must satisfy the proportion—

$$A:B:C = ma:nb:pc,$$

in which m, n, and p are simple whole numbers. The irrational values a, b, and c are fundamental magnitudes for every crystalline substance;* and Miller called these relative magnitudes the parameters of the crystals, while he called the whole numbers, m, n, and p, the indices of the respective planes. But, instead of writing the pro-

* For example, the native crystals of sulphur have $a:b:c = 1:2{\cdot}340:1{\cdot}233$.
Crystals of gypsum have $a:b:c = 1:0{\cdot}413:0{\cdot}691$.
Crystals of tin-stone have $a:b:c = 1:1:0{\cdot}6724$.
And crystals of common salt have $a:b:c = 1:1:1$.

portion which expresses the law of crystallography as above, he gave to it a slightly different form, thus:

$$A : B : C = \frac{1}{h}a : \frac{1}{k}b : \frac{1}{l}c,$$

and used in his system for the indices of a plane the values $h : k : l$, which are also in the ratio of whole numbers, and usually of simpler whole numbers than $m : n : p$. This seems a small difference; for $h\,k\,l$ in the last proportion are obviously the reciprocals of $m\,n\,p$ in the first; but the difference, small as it is, causes a wonderful simplification of the formulæ which express the relations between the parts of a crystal. From the last proportion we derive at once

$$\frac{1}{h} \cdot \frac{a}{A} = \frac{1}{k} \cdot \frac{b}{B} = \frac{1}{l}\,\frac{c}{C},$$

which is the form in which Miller stated his fundamental law.

If P represents the "pole" of a face whose "indices" are $h\,k\,l$, that is, represents the point where the radius drawn normal to the face meets the surface of the sphere circumscribed around the crystal (the sphere of projection, as it is called), and if X, Y, Z represent the points where the axes of the crystal meet the same spherical surface,* then it is evident that $X\,Y$, $X\,Z$, and $Y\,Z$ are

* The origin of the axes is always taken as the center of the sphere of projection.

the arcs of great circles, which measure the inclination of the axes to each other, and that PX, PY, and PZ are arcs of other great circles, which measure the inclination of the plane $(h\,k\,l)$ on planes normal to the respective axes; and, also, that these several arcs form the sides of spherical triangles thus drawn on the sphere of projection. Now, it is very easily shown that

$$\frac{a}{h}\cos PX = \frac{b}{k}\cos PY = \frac{c}{l}\cos PZ;$$

and by means of this theorem we are able to reduce a great many problems of crystallography to the solution of spherical triangles.

Another very large class of problems in crystallography is based on the relation of faces in a zone; that is, of faces which are all parallel to one line called the zone axis, and whose mutual intersections, therefore, are all parallel to each other. If, now, $h\,k\,l$ and $p\,q\,r$ are the indices of any two planes of a zone (not parallel to each other), any other plane in the same zone must fulfill the condition expressed by the simple equation

$$\mathrm{u}\,u + \mathrm{v}\,v + \mathrm{w}\,w = o,$$

where u v and w are the indices of the third plane, and u v w have the values

$$\mathrm{u} = kr - lq \quad \mathrm{v} = lp - hr \quad \mathrm{w} = hq - kp.$$

Since $h\,k\,l$ and $p\,q\,r$ are whole numbers, it is evident that u v w must also be whole numbers, and these quantities are called the indices of the zone. The three whole numbers which are the indices of a plane when written in succession serve as a very convenient symbol of that plane, and represent to the crystallographer all its relations; and in like manner Miller used the indices of a zone inclosed in brackets as the symbol of that zone. Thus 123, 531, 010 are symbols of planes, and [111], [213], [001] symbols of zones.

An additional theorem enables us to calculate the symbols of a fourth plane in a zone when the angular distances between the four planes and the symbols of three of them are known, but this problem can not be made intelligible with a few words.

The few propositions to which we have referred involve all that is essential and peculiar to the system of Professor Miller. These given, and the rest could be at once developed by any scholar who was familiar with the facts of crystallography; and the circumstance that its essential features can be so briefly stated is sufficient to show how exceedingly simple the system is. At the same time, it is wonderfully comprehensive, and the student who has mastered it feels that it presents to him in one grand view the entire scheme of crystal forms, and that it greatly helps him to comprehend the scheme as a whole,

and not simply as the sum of certain distinct parts. So felt Professor Miller himself; and, while he regarded the six systems of crystals of the German crystallographers as natural divisions of the field, he considered that they were bounded by artificial lines which have no deeper significance than the boundary lines on a map. How great the unfolding of the science from Haüy to Miller, and yet now we can see the great fundamental ideas shining through the obscurity from the first! What we now call the parameters of a crystal were to Haüy the fundamental dimensions of his "integrant molecules," our indices were his "decrements," and our conceptions of symmetry his "fundamental forms." There has been nothing peculiar, however, in the growth of crystallography. This growth has followed the usual order of science, and here as elsewhere the early, gross, material conceptions have been the stepping-stones by which men rose to higher things. In sciences like chemistry, which are obviously still in the earlier stages of their development, it would be well if students would bear in mind this truth of history, and not attach undue importance to structural formulæ and similar mechanical devices, which, although useful for aiding the memory, are simply hindrances to progress as soon as the necessity of such assistance is passed. And, when the life of a great master of science has ended, it is well to look back over the road he has

traveled, and, while we take courage in his success, consider well the lesson which his experience has to teach; and, as progress in this world's knowledge has ever been from the gross to the spiritual, may we not rejoice as those who have a great hope?

Although the exceeding merit of the "Treatise on Crystallography" casts into the shade all that was subordinate, we must not omit to mention that Professor Miller published an early work on hydrostatics, and numerous shorter papers on mineralogy and physics, which were all valuable, and constantly contained important additions to knowledge. Moreover, the "New Edition of Phillips's Mineralogy," which he published in 1852 in connection with H. J. Brooke, owed its chief value to a mass of crystallographic observations which he had made with his usual accuracy and patience during many years, and there tabulated in his concise manner. As has been said by one of his associates in the Royal Society, "it is a monument to Miller's name, although he almost expunged that name from it." * It is due to Professor Miller's memory that his works should be collated, and especially that by a suitable commentary his "Tract on Crystallography" should be made accessible to the great

* "Obituary Notices from the Proceedings of the Royal Society," No. 206, 1880, to which the writer has been indebted for several biographical details.

body of the students of physical science, who have not, as a rule, the ability or training which enables them to apprehend a generalization when solely expressed in mathematical terms. The very merits of Professor Miller's book as a scientific work render it very difficult to the average student, although it only involves the simplest forms of algebra and trigonometry.

Independence, breadth, accuracy, simplicity, humility, courtesy, are luminous words which express the character of Professor Miller. In his genial presence the young student felt encouraged to express his immature thoughts, which were sure to be treated with consideration, while from a wealth of knowledge the great master made the error evident by making the truth resplendent. It was the greatest satisfaction to the inexperienced investigator when his observations had been confirmed by Professor Miller, and he was never made to feel discouraged when his mistakes were corrected. The writer of this notice regards it as one of the great privileges of his youth, and one of the most important elements of his education, to have been the recipient of the courtesies and counsel of three great English men of science, who have always been "his own ideal knights," and these noble knights were Faraday, Graham, and Miller.